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A

VIEW

OF THE

PROGRESS AND PRESENT STATE

OF

Animal Chemistry.

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Animal Chemistry,

BY

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PROFESSOR OF MEDICINE AND PHARMACY, &c. &c.

TRANSLATED FROM THE SWEDISH,

BY GUSTAVUS BRUNNMARK, D. D.

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PREFACE.

THE Royal Academy of Sciences at Stockholm elects its President every half year from among its Members; and it is required, by the statutes of the Academy, that whoever has filled that office should, upon leaving it, read an Essay, or deliver a Speech, on some literary or scientific subject of his own choice. This regulation gave rise to the present Treatise, the Author of which has occupied himself very much with investigations in Animal Chemistry, and, several years ago, published a Work, in two volumes, on this new branch *of Science. In the year* 1810, *when he gave up his Presidency in*

the Academy, he chose for his subject, a View of the Progress and present State of that Science, *combining what he had himself investigated, with what he had derived from the scattered Works of other Chemical Writers.*

As very few of Professor BERZELIUS's *Publications have been translated into other languages, and none of them has, as yet, appeared in a complete state in English, the Translator of this little Work flatters himself, that he is doing a not unacceptable service to the British literary Public, at the same time that he feels great pleasure in having the honour to introduce his most particular friend to the nearer notice of those, who may already be acquainted with his name.*

If the translation, as it is hoped, has the merit of being correct in a scientific point of view, it is entirely owing to the kind assistance of two literary friends, both members of the Royal Society—Dr. THOMAS YOUNG, *and Mr.* WILLIAM ALLEN, *who have had the goodness to read through and revise the whole. Dr.* YOUNG *was well ac-*

quainted with the original, from having been engaged in abridging it for a work of his own: Mr. ALLEN's familiarity with every department of Chemistry, rendered him particularly well qualified for the appreciating the accuracy of chemical language; and the Translator takes this opportunity of expressing to both these Gentlemen his most sincere thanks, for their very valuable corrections.

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THE Royal Academy of Sciences having honored me with so distinguished a mark of its confidence, as to appoint me its President during the last six months, I have only to regret, that, unaccustomed to such an office, I should not have been able to do that justice to its choice, which I have ardently wished; but I have always flattered myself with the hope, that the Academy, aware of my good intentions, would overlook what was wanting in my abilities; and that hope has not deceived me. On giving up this day the situation, which I have held, to a Gentleman*, who, by his age, experience, and learning, is so

* W. af Hisinger.

much worthier than myself of that distinction amongst you, I shall avail myself of the present opportunity to occupy the attention of the Academy with some historical remarks on a Science, in which, by much labour and attention, I have acquired some information and experience, viz. the Chemistry of the Animal Body; and I will shortly relate the progress which it has made during these latter years. The facts which have been ascertained in this science are interesting, and the knowledge of them useful to every one; although the science, in its detail, can only be followed up by such as make this branch of Chemistry their peculiar study.

I shall not detain you with the history of Animal Chemistry from remoter times; since the relation of the mistakes of our Forefathers, and the slow progress which every science has made, interesting as it may be in the history of man, is of little or no consequence in examining the present state of any particular science.

Our forefathers began very early to explain the phenomena of living nature, from observations made upon inanimate matter: this gave rise to particular Sects in medicine, which then, as now,

were doomed to see their theories overturned by experience. The very intricate composition of animal matter, and the innumerable productions which, in various ways, may be obtained from it, made it almost impossible, in the infancy of chemical knowledge, to produce any accurate analysis of it. It was not till after the discoveries of a BLACK, and the lucid explanation of such phenomena, as before his time were either unknown or misunderstood.—It was not till after the reiterated and solid experiments, of SCHEELE, and of PRIESTLEY, and the establishment of the new theory, which LAVOISIER, assisted by the labours of these predecessors, had given to chemical science, that it was possible to begin the examination of the interior œconomy of the animal body with any hope of success. The facts, discovered by BLACK and LAVOISIER, were now applied to Animal Chemistry, sometimes, it is true, with good effect, but often with greater confidence in the general application of the new doctrine, than experience afterwards sanctioned. In this manner, the first scattered Works were produced on this subject, the aggregate information of which, constituted the still new and infant Science of Animal Chemistry.

The constituent parts of the animal body are altogether the same as those found in unorganised matter, and they return to their original unorganic state by degrees, partly during the progress of life, partly when the body, after death, undergoes its final change. And independently of this, there exist processes between the unorganic constituent, or elementary particles within the animal body, which have sometimes not the least resemblance to those we see in unorganised matter. We may consider the whole animal Body as an instrument, which, from the nourishment it receives, collects materials for continual chemical processes, and of which the chief object is its own support. But, with all the knowledge we possess of the forms of the body, considered as an instrument, and of the mixture and mutual bearings of the rudiments to one another, yet the cause of most of the phenomena within the Animal Body lies so deeply hidden from our view, that it certainly will never be found. We call this hidden cause *vital power;* and like many others, who before us have in vain directed their deluded attention to this point, we make use of a *word* to which we can affix no idea. This *power to live* belongs not to the constituent parts of our bodies, nor does it belong to them as an

nsrument, neither is it a simple power; but the result of the mutual operation of the instruments and rudiments on one another—a result, which varies as the operations vary, and which often, from small changes and obstructions, ceases altogether. When our elementary books inform us, that the vital power in one place produces from the blood the fibres of the muscle; in another, a bone; in a third, the medulla of the brain; and in another again, certain humours, which are destined to be carried of; we know after this explanation as little as we knew before.

This unknown cause of the phenomena of life is principally lodged in a certain part of the Animal Body, viz. in the nervous system, the very operation of which it constitutes. The brain and the nerves determine altogether the chemical processes which occur within the body: and although it cannot be denied that the exercise of their functions tends to produce chemical effects; yet we are constrained to confess, that the chemical operations therein are so far beyond our reach, that they entirely escape all our observations. Our deepest chemical researches, and the finest discoveries of later times, give us no information on this subject. Nothing of what Che-

mistry has taught us hitherto, has the smallest analogy to the operation of the nervous system, or affords us the least hint toward a knowledge of its occult nature. And the chain of our experience must *always* end in something inconceivable; unfortunately, this *inconceivable something* acts the principal part in Animal Chemistry, and enters so into every process—even the most minute, that the highest knowledge which we can attain, is the knowledge of the nature of the productions, whilst we for ever are excluded from the possibility of explaining how they are produced. Permit me here to shew by an example the embarrassment of the student of Animal Chemistry on all occasions, when the inconceivable nervous system exercises its operations: It is well known that blood, which is always formed from the food of the animal, is the raw material out of which the body recruits and re-produces its parts; and that this blood, which is every where of the same nature, is conveyed through the arteries to the different parts of the body. From this blood the kidneys form urine; the glands near the ear and under the tongue, saliva; those in the breasts of women, milk; and so forth: all which are humours of the most different nature. The most acute anatomical investigation has proved,

beyond all doubt, that the vessels in these parts, while they extend themselves, proceed in an uninterrupted course, without communicating with any others—that no foreign humours which could affect the blood, have access to them; and that consequently the blood is not exposed to the influence of any mixed chemical agency. But what is it that here effects the chemical process, which, from the very same particles of the blood, forms those of saliva, milk, and urine? It cannot be form and flexure of the vessels, since that can only cause a greater or less delay; and that this alone cannot determine the formation of the secreted matter, common Chemistry will shew. Consequently, there remains only the influence of the nerves, which enter into these parts, and which determine as well the nature of the secreted matter as its quantity; but until our experiments on unorganised matter shall have furnished us with a chemical phenomenon, which has any analogy with the operations of the nerves on these occasions, we shall never be able to discover the laws of those operations, nor explain the intimate nature of these processes. And if the knowledge of the transformation of the blood into other humours, which knowledge does in itself bear an analogy to chemical phenomena

in general, is so deeply hidden from our view, how shall we attempt to explain the renewal of the solid animal parts, whereby the body is supported during the constant exchange of its elements? But still more astonishing are the operations of the brain. How amazing, that our thought, even in its sublimest flight, and when it penetrates the most hidden recesses of nature, should depend on a previous chemical process, which, if in the least disturbed as to its correctness, would distract this very thought, change it into madness, or make it cease altogether; and yet this is an incontrovertible truth. But is it not probable, that human understanding, which is capable of so much cultivation, which has calculated the laws of motion for distant worlds, and explored in so many instances the beauty and wonders of surrounding nature, and even attained a degree of perfection, the summit of which is concentrated in God, may one day explore itself and its nature? I am convinced it will not.

To give an account of all the attempts which have been made to explain on this head, what we nevertheless are as ignorant of as before, would be to write a long and useless book; for we have seen nothing but mere speculation, with-

out the least experience for its foundation, and without explaining satisfactorily a single phenomenon.

One conjecture however deserves our notice, since it is deduced from experience in unorganised bodies, and might therefore, after previous examination, have been approved or rejected, viz. that the nerves were instruments for an electric process, which determined as well the movement of the body, as the nature of the secretions and re-production of parts. The power of electricity to produce a quick contraction of the muscles, gave occasion to compare it to the operation of the *will;* and the conclusion drawn from it was, that the latter was nothing else than an electric discharge between the nerve and the muscular fibres. GALVANI, the discoverer of that modification of electricity, which now bears his name, has more than any other defended this doctrine; but although he has not wanted followers, the insufficiency of his hypothesis is now pretty generally acknowledged. THOMAS BUNZEN, a Dane, laid bare the crural nerve of a frog, and endeavoured to cut it obliquely in such a manner that the medulla could be placed in immediate contact with a part of a muscle. He then formed a pile of from

ten to twelve prepared frogs, in the following manner: nerve, muscle, a sponge dipped into a solution of muriate of ammonia, nerve, muscle, &c. and obtained from this pile distinct signs of galvanic action. By this he wished to prove, that nerves and muscles may act as electromotors. But it is difficult to determine what value such experiments can have in reference to the subject in question; for it is more than probable, that electricity may be produced here, and act on the expiring life, without its proving that the action of the nervous system of the living animal bore the least analogy to the experiment. Lately, EVERARD HOME has tried to explain the animal secretions, from the changes which the discharge of the electric pile produces on liquids; but, if on the one hand, it be found by future experiments, that electricity identifies itself with chemical affinity, and consequently a change of composition is not to be expected without the co-operation of electricity; yet, on the other hand, the effect of the pile, both on animal and unorganic liquids, has nothing at all analogous with the secretions; and by applying this chemical agent to explain the subject, we gain not the least information.

Among those, who have laboured in vain on this field, I ought also to mention the well-known German author, REIL. He supposes, that in the nerves there is a matter, analogous to galvanism, which through a kind of electric atmosphere, operates at a small distance, and thereby he has revived the idea of an *aurea nervea.* The manner in which he explains his hypothesis, and the arguments he introduces in support of it, constitute an entertaining essay, but does not increase the sum of our real information.

It is fruitless to expect to obtain any information on this subject, by any chemical analysis of the matter of the nerves and the brain. Our experiments convince us sufficiently, that the operation of the nervous system is not performed by a mutual decomposition of its medulla, and the part on which it operates; for, by tying up any small but essential nerve, we have found, that the greatest disorders have arisen in the œconomy of the animal, and continued as long as the ligature remained, although the nerve below the part tied always retained the same quantity of nervous substance as before. Again, if the ligature is loosened, and the continuity of the medulla restored, the disorders will cease. And why is this

continuity so necessary in a channel, the contents of which always remain on the same spot? It is clear that this indicates an effect by means of transmission, like that of electricity; although, what we hitherto know about electricity, cannot here be applied in explanation.

In this state of our information, I consider it as no small merit in a lover of the science, if he distinctly lays open what is really known, and determines with equal distinctness, what is yet unknown to us, without filling up the chasm with conjecture. Problematical tenets, they say, are conductors to truth; and I do not altogether deny it, when they are proposed for examination by themselves; but when in the accumulation of scientific knowledge, they are mixed with the mass of facts, they often lead even the intelligent reader astray, who afterwards cannot, without labour and application, get rid of the illusion. Animal Chemistry is more exposed to this intermixture of hypothesis than any other science; partly because much remains still to be explored, and partly because there is still more, which certainly never can be discovered. It might indeed be possible, by applying to this science the many more or less ridiculous medical theories about the

nervous system and the intimate nature of its operations, to compose *a whole*, which should have a truly scientific appearance; but all, that in latter times has been tried this way, from what has been called a higher philosophical point of view, has only done honour to the imagination of the author, without bringing the human understanding a hair's-breadth nearer the truth.

Those of our contemporaries, who have subjected parts of the nervous system to chemical experiments, are THOURET, FOURCROY, JORDAN, and in some respects, even BICHAT. The former have given us analyses of the matter of the brain, which, considering the time when they were published, are of real merit: they constitute all that Animal Chemistry can yet shew relative to this noble organ; but in the present state of the analytical part of the science, they need to be revised and corrected. BICHAT has examined the membrane of the nerves (the *neurilema)*. He was, properly speaking, not a Chemist; but in order to improve his physiological works, he tried on most parts of the animal body the effect of general chemical agencies, such as that of air, water, alkalies, and mineral acids; and the experiments thus made on the "neurilema" are all that we know of it

in a chemical sense. The discovery of the possibility of dissolving, by means of caustic alkali, the medullary part of the nerve, so that its membrane should remain as a hollow tube, has afforded a good opportunity for separating the membrane, and has given us some information concerning the nature of the channel which the neurilema forms.

Next to the nervous system, the principal part in the animal body is acted by the blood and the vessels by which it is circulated. The extremities of these vessels are interwoven with the last ramifications of the nerves, and perform in this union, at the expence of the blood, all the processes of the animal body. The attention of Chemists was very early directed to the blood; and its different properties in certain diseases, induced them to make several experiments with it. HALES tried to determine the quantity of air which is disengaged in the distillation of the blood. LEMERY and MENGHINI burnt it to ashes, found iron amongst its constituent parts, which the latter even believed himself able to extract from dry blood with the magnet. HOFFMAN examined the different matters which are separated in the coagulation of blood. LANGRISH, CHEYNE, and SCHWENKE, published for their

time, tolerably explicit analyses of the blood. GAUBIUS surpassed them all in accuracy.—— ROUELLE, the younger, determined pretty distinctly the salts which are contained in it. HEWSON described with precision several of the qualities of the blood. BUCQUET examined the constituent parts of the cruor: and lastly, we have from DEYEUX and PARMENTIER, in answer to a prize-question from the Medical Society at Paris, an explicit analysis of the blood, as well in its healthy state, as in certain diseases. FOURCROY and VAUQUELIN added, some years afterwards, an investigation of the colouring matter of the blood; but all that we have gained, since the excellent work of DEYEUX and PARMENTIER was published, has almost only been the clearing away of some errors, without much positive additional information. I have also attempted to give a detailed analysis of the whole mass of the blood; and, assisted as I have been by the improved state of Chemistry in later times, I may perhaps have succeeded somewhat better than most of my predecessors, in tracing the constituent parts of the blood, even such as were unknown to them; and also in determining in a more decided manner the characters of those that were known; so that in any future analysis of other humours or parts of

the animal body, they may be ascertained by their chemical properties. Thus, for instance, I have shewn that the fibrin of the colouring matter and the albumen may be combined with mineral acids in excess, and form peculiar and insoluble compounds, but that when the superfluous acid is washed away, they are soluble in water—that these matters are easily dissolved in acetic and in phosphoric acids, and that these acids prevent the coagulation of the blood, by heat—that the fibrin, by being boiled with water, disssolves in a small degree, and that the rest shrinks together, and is insoluble in acetic acid—that they all three, by the influence of alcohol and æther, are changed to a certain degree, into peculiar kinds of fat, which, according to the menstruum, have different pungent smells, and other differences. The ingredients hitherto unknown, which I have found in the blood, are alkaline lactate of potash, and some peculiar animal matters, which in all the humours of the body are found to accompany the lactate; and which, in my opinion, owe their existence in the blood to the absorption of those decayed parts of the body, which are destined to be separated by means of secretion. I have also succeeded in correcting several mistakes of my predecessors. It was be-

lieved from De Haen's statement, that blood contained glue, of that kind which is produced when bones or cartilages are boiled with water; but I have proved that glue is not found within the animal body, and that De Haen, and all after him, have considered as glue, the albumen, in a half coagulated state. Among the immediate constituent parts of the blood, some authors have even counted sulphur, because blood, when evaporated in silver vessels, blackens the silver. This conclusion, however, is incorrect, because the sulphur has belonged, as a constituent part, to the albumen, and has been disengaged through the combined destructive effect of boiling, and of the caustic alkali, on the albumen. Deyeux and Parmentier believed that the red colour of the blood was a solution of iron in the free alkali of the blood. Fourcroy and Vauquelin endeavoured to prove, that it was a solution of red subphosphate of iron, in albumen. They found that albumen, or serum, which was triturated with this subphosphate before it was dry, dissolved it, and assumed itself a red colour, and that this red colour was still more heightened by caustic alkali. According to these experiments, the colouring of the chyle in the air consisted in the change of the phosphate of iron, from a neutral phosphate of the

the protoxide, to a subphosphate of the peroxide. With the greatest diffidence of myself, I have repeatedly tried the experiments of those Chemists, the most celebrated of all Europe: and by finding my results invariably disagree with theirs, I have been compelled to consider their statement as a mistake, and to declare, that, in regard to the manner in which iron is united to the colouring matter of the blood, we know as little now, as when iron was first discovered in it. I have endeavoured to shew, that the colouring matter, much as it resembles albumen, cannot itself be albumen; and that as LEEUWENHOEK and HARTSOEKER proved long ago, by microscopical observations, it is not dissolved in the blood, but floats in it in a suspended state; for if the coagulated cruor is triturated in serum, part of the colouring matter is thereby separated, and the serum assumes a red colour; but if it is suffered to settle in a cylindrical glass, the colouring matter slowly precipitates itself to the bottom, and the serum above becomes clear, as before.

I have proved, that metallic oxides, particularly those of iron, may, in a certain degree, be dissolved by the serum, and thereby, more or less, change its colour; but that none of them imparts

to it the colour of blood, and that the serum, thus impregnated with iron, is entirely destitute of the intrinsic characters of the colouring matter. As none of our most delicate tests of iron discover its presence in the colouring matter, I thought myself entitled to conclude, that iron cannot be found in it in a saline form; and, as we find it impossible, even with the strongest acids, to extract from the blood, or from its charcoal, either the iron or the earthy phosphate, which are so abundantly contained in the ashes of blood, it follows of course, that neither of these substances exists there in the state of a salt; from which it is very probable, that blood contains the elements of these salts, united in a manner different from their combination in the salts. From this circumstance, I further concluded, that the subphosphate, or *bone-earth,* which was supposed to be contained in the blood, did really not exist there, for I found that it could not be extracted from dried blood with any diluted acid; on the contrary, that *bone earth* must always be a production of the decomposition of the immediate constituent parts of the blood, and that it is generated just on the very spot where its presence is required.

On the cause of the coagulation of the fibrin out of the body, many experiments have been made. Blood has been carefully kept at the same temperature, and exposed to air containing no oxygen gas, and also in an exhausted receiver. Blood has been frozen rapidly, and again thawed, or mixed with water; but, in all these experiments, it has, sooner or later, coagulated. The cause of this coagulation remains entirely unknown to us; and a conjecture has been made, that it is only its motion in the vessels which prevents it. Some Experimenters have ascribed to the fibrin a vital irritability, in consequence of the tremulous motion perceivable in small drops of blood, when exposed to the effect of the electric column; but this idea was proved by HEIDMAN to be completely erroneous, and he shewed that the motion proceeded from the shrinking of the fibrin itself, when suddenly coagulated. The chemical examination of the fibrin, the colouring matter, and the albumen, has discovered that these three substances very nearly resemble one another in chemical properties; their composition must therefore be nearly similar, and they are capable either of being changed, by means of small alterations within the living animal, the one into the other, or of being employed to

produce the same substances in the excretions, or in the re-production of solid parts, instead of those which are decayed, or worn out.

On accurately comparing human blood with that of a bullock, I have found an astonishing resemblance between the two.—The same specific constituent parts in both, the same proportions, and nearly the same chemical characters, make it easy to explain the successful results of several experiments that have been made, to transfuse the blood of herbivorous animals into human bodies, from which the blood had been drawn at the time. I observed, however, a remarkable difference in certain characters of the constituents of the blood in man, from what I found in that of the bullock. The fibrin, as well as the colouring matter, and the albumen, from human blood, after they are dried, are much more easily burnt to ashes; and the charred human blood requires neither so strong, nor so protracted a heat, to be entirely reduced to ashes, as that of the bullock. This difference in the facility of burning, indicates clearly a greater proportion of nitrogene in the constituent parts of the bullock's blood, which is still more clearly proved by the circumstance, that the charcoal of the

blood of the bullock, when burnt slowly, constantly gives off carbonat of ammonia, although it has been heated in an open vessel, and freely exposed to the air. This indication of a greater quantity of nitrogene in the constituent parts of the bodies of herbivorous animals, than in those of the human body, is the more unexpected, as the food of man, in general, contains more nitrogene; whereas, on the contrary, nitrogene, which has hitherto been considered as an elementary body, is found but in small proportions in those vegetables, which constitute the food of the bullock. Our information on this subject will probably be considerably increased, by an examination of the so much contested nature of nitrogene.

The blood separates itself at the finest ramifications of the arteries, into a *coloured* portion, which returns the suspended particles of the colouring matter through the veins, and a *colourless* portion, which penetrates the finest ramifications. These ultimate ramifications of the arteries, we call, after BICHAT, *capillary vessels.* The colouring matter changes, on this occasion, its colour, and turns dark brown, or blackish; but it is entirely unknown to us what influence

this changing of colour may produce on the colourless fluid, which is conveyed by the capillary vessels. As the colouring matter is not dissolved, but only mechanically mixed with the blood, we may consider the separation of the arterial blood into coloured and uncoloured, as a process of filtration, which only admits the colouring matter to penetrate into the veins, while, on the other hand, the serum alone is forced into the finest channels.

It would be of the greatest importance to Animal Chemistry, to examine the serum, in the state in which it penetrates the capillary vessels; but I cannot conceive how it would be possible to collect it, even in a very small quantity. It is probable, that this humour is the common serum, which also contains dissolved fibrin; and if this supposition be correct, it follows, that the fibrin within the vessels must also be dissolved in the serum, and not belong to the suspended colouring matter. Such I have always considered the composition of the blood; but, except the confirmation we receive, by examining the humour of the absorbing vessels, I have not been able to discover any experiment to confirm, or refute, this supposition.

The blood vessels have, as yet, been but little examined as to their chemical properties; and, with the exception of BICHAT'S experiment of macerating their various membranes, we have no investigation on the subject. The fibrous membrane of the arteries, which, unquestionably, is the most remarkable of all, has long been considered as composed of annular muscles. This was the opinion of HALLER, and on this supposition he has founded his theory of the *pulse,* which to this day is adopted in all our elementary books. JOHN HUNTER disapproved HALLER'S idea of the muscular action of the arteries, as being the cause of their pulsation. BICHAT tried to irritate the arteries of living animals, with such chemical and mechanical stimuli as affect the muscular fibre, but without being able to excite the least perceptible change in their motion; and he declared, in consequence of these experiments, that the pulsation originated only in that of the heart; that it did not consist in dilatation, but was only a motion from its former place, or, as he himself called it, a *locomotion*.. The chemical examination of the fibrous membrane of the arteries, now became of peculiar consequence, as the only possible means to decide how far the fibre of the artery was of the same nature as that

of the muscle. I undertook this examination, and obtained very satisfactory and decisive results. In consequence of the experiments thus made, it is beyond all doubt, that the fibrous membrane of the arteries cannot be a muscle; for while the latter is soft and flaccid, and contains more than three-fourths of its weight of water, the artery is dry and very elastic; the muscular fibre possesses the same chemical properties as the fibrin of the blood; for instance, that of being soluble in acetic acid, and of forming scarcely soluble compounds with sulphuric, nitric, and muriatic acids; but the arterial fibre has altogether opposite qualities, viz. that of not being soluble in acetic acid, but pretty easily soluble in mineral acids, diluted with water to a certain degree, from which solution it cannot be precipitated by means of alkali, or alkaline prussiats, which are the tests for the acid solution of fibrin, &c. Consequently, as the arterial fibre neither has the structure of a muscle, nor its chemical properties and composition, it cannot be a muscle, nor perform the functions of a muscle, which is, besides, sufficiently evident from its elasticity. This elasticity in the arteries, however, compensates fully the muscular power. Haller's description of the pulse, is of course correct, not-

withstanding his opinion on the cause of the arterial contraction, has been proved to be incorrect. Bichat's idea, on the contrary, that the pulse did not consist in a dilatation of the arteries, but only in a *locomotion,* occasioned by their numerous inflections, when the heart presses the blood, cannot but be incorrect, since it is contrary to the laws of hydrostatics.

As chemical analysis has sufficiently proved that the fibrous membrane of the arteries is not a muscle, and, consequently, cannot exhibit a spontaneous contraction; and, as we clearly perceive from its elasticity, that it must be dilated during the systole of the heart, and resume its original size during the diastole; it follows, that the quickness of the pulse in the same individual, can never vary in different parts of the body. All other disparities, except this, may be possible.

Several medical authors have related cases, where this unequal velocity is said to have been observed; but we must consider these observations as mistakes, after we have seen the impossibility of the existence of such cases. A decision of this long-contested question is of the greatest consequence to the Medical Science,

since it convinces us that spasms cannot exist in the greater arteries, and that all the aberrations in the circulation of the blood, generally ascribed to this cause, altogether relate to the unquestionably muscular heart, with its auricles, and in some degree to the muscular fibres, which surround the extremities of the venæ cavæ.

I mentioned, that the final ramifications of the arteries, on account of their fineness, are called capillary vessels. The anatomy of these vessels is almost unknown, and the manner in which they terminate is a complete secret. It has hitherto been impossible to institute any chemical analysis of their integuments, as they cannot be separated from those parts with which they are interwoven. These vessels, in all probability, have a peculiar power slowly to carry forwards the humours contained in them; but the mechanism of this process will probably long remain a secret. It is in these vessels that the inexplicable processes of secretion and re-production of solids is performed by means of the co-operation of the nerves. The same nature, which is incomprehensible to us, when extended to immensity, escapes no less our penetration, when contracted into too narrow limits. At both these extremi-

ties a boundary is set to our experience, which succeeding ages may extend, without ever being able to comprehend the whole.

The process, which in respiration changes the dark venous blood into crimson coloured blood, was first examined by CIGNA, and not without success; and after our great SCHEELE had taught us the composition of the atmospheric air, and demonstrated as well the necessity of one of its constituents for the support of life, as the insufficiency of the other, the change of the air in the lungs was explained by LAVOISIER, MENZIES, and GOODWYN, in a very satisfactory manner. They found, that oxygen gas was consumed, and that its place was filled up by carbonic acid gas; as also, that the expired air contained a very considerable quantity of aqueous vapour. From this circumstance, LAVOISIER concluded, that the dark venous blood contained a combination of carbon and hydrogene, which imparted to it the dark colour; and which, when exposed to air, became oxygenized, and formed carbonic acid and water, whereby part of the water, which accompanies the expired air, was produced; while the other part evaporated from the humid membrane of

the lungs. He endeavoured to state the quantities of these productions; but the numbers he has given us exceed, in some degree, the due medium, because, in his days, the quantity of oxygene in the air was taken to be greater than it really is, by which means his eudiometric experiments could not be sufficiently accurate. He found further, that no nitrogene was absorbed by the blood.

Experiments were now made with other kinds of air, besides the atmospheric; and among the many, who have laboured in this field, Dr. BEDDOES has particularly distinguished himself. He tried to cure certain diseases, by means of inhalation, and gave us several instances of apparently successful results. However, a multiplied experience has shewn, that less has been gained from these inhalations than was originally believed. As a concomitant result from these experiments it was found, that hydrogene, as well as nitrogene, might be inhaled without any deleterious effect in the beginning; and that the respiration of hydrogene gas produced a cheering effect, somewhat like that of spirituous liquors. It was, however, requisite that both these gases should be in a pure state. All other kinds of gases were found to be

noxious, and even destructive. BEDDOES employed in his experiments HUMPHRY DAVY, a young man of promising genius, who has since far surpassed his instructor in celebrity and merit. DAVY discovered the intoxicating power of the nitrous oxyd gas, and shewed, that it was absorbed, during respiration, by the blood, to which it imparts a purple colour, and extricates from it part of the gases it had formerly absorbed. He afterwards extended his experiments to respiration in atmospheric air; and it appeared to him, that the blood actually absorbed part of the nitrogene in the air, so that about three or four cubic inches were absorbed every minute. HENDERSON and PFAFF repeated his experiments with similar results; a mistake, however, has since been discovered in these experiments, in consequence of not being acquainted with the laws, which regulate the mixture of gases with liquids. About this time, however, they were discovered by JOHN DALTON, an ingenuous natural philosopher, who soon afterwards published his experiments. One of these rules is, that, when a liquid comes in contact with a gas, it absorbs a determined proportion of the gas; and if it then comes in contact with another gas, it absorbs also a quantity of that, but emits, at the same time, a part of what it had absorbed

before, till the gas over the liquid, and the part absorbed by it, arrive at a certain degree of æquilibrium. As in all these experiments, the very same respired air was constantly inhaled, it follows, that the relative quantity of nitrogene in the air must have been increased, and thereby produced a double cause of mistakes, partly from the gas, remaining in the lungs, containing more nitrogene; and partly, from the circumstance, that in proportion as the air in the lungs near the blood contained more nitrogene, the blood itself, or rather the water in the blood, must absorb a new quantity of nitrogene gas, in order to approximate to perfect saturation with this gas; just as, on the other hand, the blood, during the respiration of gases, that contain no nitrogene, must continually return part of the nitrogene, which it had absorbed before, which circumstance experience has also fully confirmed. The cause of this mistake, therefore, did not originate in an incorrect or hasty experiment, but was the necessary consequence of the state of the science at the time. The experiments on respiration were lastly repeated by two English Chemists, Allen and Pepys, on a larger scale, and with a precision, which far surpassed all examinations hitherto made. In these experiments they had the

opportunity of using the excellent large gasometers of the Royal Institution in London; and the principal result of them was, that the volume of the air, during the respiration, is so inconsiderably diminished, that the real absorption hardly amounts to more than two thirds per cent. of its volume. On the other hand they found, that the carbonic acid gas, just produced, fills precisely the room of the consumed oxygene gas. As it was known already, that oxygene gas, during its transformation into carbonic acid gas, does not alter its volume, and that consequently 100 cubic inches of oxygene gas, in which carbon had been burnt, produces exactly 100 c. i. of carbonic acid gas, it was completely proved by ALLEN and PEPYS'S experiments, that no hydrogene is oxydated in the lungs—that the oxygene is consumed only by the charcoal—and that, for the rest, the blood, according to all appearance, is not oxydated, but only emits carbon, (or is decarbonized).

These chemists could not observe any other alterations in the composition of the expired air: it had lost no nitrogene, and acquired no other aerial substance, than carbonic acid gas. This constituted about 8½ p. c. of the volume of the

air, which was increased to 10 p. c. if the same air was repeatedly respired; but it never exceeded this quantity however long the respiration of the same air was continued. Again, on those occasions, where the respiration was performed with some difficulty, more oxygene was absorbed, than carbonic acid gas produced. These Gentlemen had the kindness to send me a copy of their essay, inserted in thePhilosophical Transactions for 1808; in consequence of which, I took the liberty to propose to them some further experiments; for although it was easy to explain the loss of nitrogene gas in DAVY'S experiments, and there was consequently no reason to doubt the correctness of their observation, that no nitrogene disappears during respiration, yet I had, for some time back, entertained the idea, that nitrogene might possibly be absorbed by the blood of herbivorous animals, whereby their bodies became provided with the nitrogene, which was wanting in their food, and I therefore proposed to Messrs. ALLEN and PEPYS to examine also the respiration of herbivorous animals, with respect to the absorption of the nitrogene. They executed this experiment, and obtained altogether unexpected results. They made use of guinea pigs, which were placed in a gasometer, and kept there for about an hour,

after which, the air was examined. When using atmospheric air, they found the quantity of nitrogene gas entirely undiminished, and the consumed oxygen gas compensated by carbonic acid gas, just as in man. They then caused these animals to respire pure oxygene gas, in an apparatus, so constructed, that the respired gas could be exchanged for new, and that, in which the animal had respired, be separated for examination.—This was found to contain a large proportion of nitrogene gas, which, however, was decreased more and more in the succeeding portions. They now mixed 78 parts of pure hydrogene gas with 22 parts of oxygene gas; and in this artificial atmosphere, they confined a guinea pig for an hour, having previously ascertained the volume with minute exactness, and the same result was again obtained: the expired air was mixed with nitrogene gas, in a decreasing proportion; but, the quantity of nitrogene gas, obtained in this manner, exceeded, in some experiments, the bulk of the animal. They found, besides this, that the animals, after the lapse of an hour, became sleepy, without any perceptible sign of illness; and that, during this period, less carbonic acid gas was produced. As in these experiments, more nitrogene gas was exhaled, than the fluids of the

animal, at the moment of its introduction into the mixed gas, could possibly contain, in the state of absorbed nitrogene gas, it appears, that, after the absorbed nitrogene gas was exhaled from the blood, according to the above-mentioned law, for the combination of mixed gases with liquids, a fresh quantity of nitrogene gas was supplied at the expence of the constituent parts of the blood, which seems to have a constant tendency to provide itself with nitrogene gas; and this must again be exhaled, in order to divide itself between the blood and the gas in the lungs.—Should this conjecture be supported by future observations, it will, nevertheless, always be looked upon as a peculiar and uncommon chemical process, that nitrogene gas should be disengaged without any particular change in the composition of the blood, when we consider, that nitrogene is in general disengaged only by operations more or less destructive, such as, for instance, by the effect of mineral acids.

This is at present the extent of our information about the change of the air by respiration. The influence which the air exercises on the blood, and the changes which are thereby produced in the composition of the blood, are, as yet un-

known. We have only been able to ascertain, that the dark blood turns red; and we conclude, from the quality of the respired air, that the dark blood has lost a portion of its carbon; but whether any of the immediate constituent parts of the blood have become altered in their qualities, is a subject not yet examined, although it is probable, that, by a chemical analysis, very interesting results might be derived from the comparison between venous and arterial blood. It has been generally believed, that every part of the blood is influenced by the air;—that it absorbs oxygene, and exhales carbonic acid gas; but this is not the case. Blood, in which the colouring matter is still contained, absorbs oxygene gas very quickly, when out of the body and shaken in atmospheric air; it also retains at the same time some part of the carbonic acid, thereby produced; on the other hand, serum, when destitute of colouring matter, does not change the atmospheric air before it begins to putrify.

The principal effect on the air is produced by the colouring matter, and as this matter does not penetrate any of the re-producing, and but few of the secreting capillary vessels, it appears as if the principal object of the colouring matter, were

the production of animal heat. **Crawford** stated, in consequence of his experiments, that arterial blood was possessed of a greater specific heat than the venous, in the proportion of 115 to 100. Taking this statement for granted, it follows, that when the venous blood turns red in the lungs, it must be cooled about 5 degrees, in case it be not heated by the very process which renders it arterial. The ideas, about the distribution of heat, through the body, were at first very vague, and the lungs were considered as a stove, in which the heat was disengaged, and afterwards carried with the blood to all parts of the body. According to **Crawford's** idea, it seems as if the arterial blood, in order to maintain the same temperature, as when venous, did exactly want that quantity of heat, vhich is engaged from the air, by the oxygene gas being changed into carbonic acid gas; and when the arterial blood again becomes venous in every part of the body, the retained heat becomes disengaged, and restores what has been lost by means of transpiration, and by the access of the ambient air, whereby the body always preserves the same degree of heat. If the colouring matter is the principal cause of this, it is easy to perceive, wherefore the body, after abundant losses of blood, is more

cold; and wherefore bleeding, even in asthenic fevers, lessens the violence of the disease. On these occasions, the effect cannot be owing to the diminished bulk of the humours alone, since the former diameter of the vessels is soon restored by the absorption of new fluids, from the alimentary canal; but the diminution of the colouring matter of the blood, and the production of heat, which depends on it, must also have a considerable share in it. Practitioners in physic will, no doubt, recollect circumstances in the course of their experience, which will give still greater probability to this idea.

By ascertaining the quantity of carbonic acid, which is daily formed by respiration, we may pretty nearly determine the quantity of heat, which is necessary for maintaining the equable temperature of the body. ALLEN and PEPYS found, that a middle-sized person expires in 24 hours, 39,534 cubic inches of carbonic acid gas, which, when weighed, makes 19,683 grains, or 39 ounces of carbonic acid. These contain 11 ounces 1 dram of carbon; from which it follows, that a middle-sized person wants, within 24 hours, as much heat for keeping the body at + 32° * as

* The Author uses the centigrade thermometer.

a pound of charcoal would disengage during its combustion. It ought also to be observed, that the carbon in the blood, as being in a liquid state, may possibly produce more heat from its combustion, than when in its solid state. All these calculations, however, have so little claim to be regarded as exact, that they are made rather with a view to approximate to the truth, than to be relied upon as accurate. For my part, I must confess, that if the observations of ALLEN and PEPYS be correct, it will be very difficult to conceive how the body can compensate the extraordinary consumption of carbon, which, besides what is separated in other places, presupposes at least from 8 to 10 pounds weight of food within the day, which is more, by far, than any person generally consumes.

Although the changes, which the blood, during respiration, undergoes in the lungs, according to all appearance, resemble those, which are produced in the blood by the air out of the body; an influence, in this process, has also been ascribed to the nervous, system, without which, it could not take place. DUPUYTREN, in an experiment on horses and dogs, divided the eighth pair of nerves, near the œsophagus, and ob-

served, that the animal, though respiration was complete, died within a short time for want of oxidation. If an artery was opened, and the nerve afterwards divided on one side, the blood, which issued from the artery, assumed for a few moments a darker colour, but was again reddened. If the nerve was divided on both sides, the blood which flowed from the artery became venous, and the animal died, the fine epidermis on the interior side of the nose and mouth becoming blackened. Again, if instead of this, he only compressed the nerves, the arterial blood became dark, and continued in that state as long as the compression lasted, but assumed again a crimson colour as soon as the compression ceased. These experiments prove, in a decided manner, the influence of the nerves upon the change of the blood in the lungs, if they were but correctly performed and related. In the mean time, it is well known that Baglivi and Bichat have made similar experiments, and drawn from them quite different results. Besides this, one of Dupuytren's countrymen, Ducrotay de Blainville, has repeated his experiments, and found that the animal indeed, dies sometime after the eighth pair of the nerves have been divided, but from quite another cause than the cessation of

the change in the blood during respiration. This was afterwards still further examined by EMMERT, with all the accuracy we could wish in a skilful experimenter, and he has satisfactorily proved, that the dividing of the eighth pair of nerves has no immediate influence on the change of the blood in the lungs, but that it affects the respiration, which, by degrees, becomes more and more uneasy; after which, the blood in the arteries begins to become dark, so that, when the animal, after several hours, dies, it is found to have venous blood in the arteries, because the respiration has ceased.

Of the difference in the blood in different ages, and during different diseases, we know hardly any thing. It has been supposed, that the blood of the fœtus underwent, in the placenta, a process not unlike that, which takes place in the lungs after birth, and that it was returned through the veins of the navel-string; but credible authors have asserted, that the eye cannot distinguish between the arterial and venous blood of the fœtus. The chief object of the process in the lungs, is the maintaining of the animal heat, but the fœtus derives its temperature from the surrounding medium, and consequently wants no source of heat

of its own; it would, on the contrary, by such a source, be constantly kept at a much higher temperature, than the mammalia in general have. This circumstance is, of course, decisive against the change of the colouring matter in the placenta, although we must confess, that the stay of the blood there must answer some important purpose. FOURCROY has communicated some observations on the blood of the fœtus; but they seem to bave been made by chance, and not to be deduced from any experiments.

The dissimilarity of the blood in certain diseases was examined by DEYEUX and PARMENTIER, and the result of their examination was, that the difference from the healthy state was so inconsiderable, that in the state of the science at that time, they were hardly to be ascertained. DUPUYTREN, THENARD and NICOLAS, have made experiments with diabetic blood, and proved, that not the smallest portion of the sugar, which is so abundantly found in the urine of those afflicted with diabetes, can be discovered in the blood.

What I have here stated concerning the blood and respiration, relates chiefly to man. We have

no comparative experiments on the blood of the brute creation; and of this, Animal Chemistry has taught us nothing beyond the external characters, which, in Zoology, constitute a part of the distinctions of the different classes of animals. On the respiration of birds, and amphibia, no experiments have been made. We only know, that birds are exceedingly delicate as to air, and that in the same atmosphere in which a bird dies, a mouse may live without any perceptible inconvenience. On the respiration of fishes, we are furnished with more experiments. It is certain, that the fish oxidates its blood in the gills, at the expence of the oxygene gas, which the water contains to the amount of about $\frac{1}{100}$ of its volume. But this kind of respiration is not of so much consequence to fish, as to the mammalia, since fishes may continue alive for several days in water, which is void of air; but they die at last, if air is not admitted, and it cannot be observed, that the least decomposition of the water takes place by their respiration. Fishes have a vessel, which is called the air bladder, and which has, though probably not correctly, been considered as partaking in the operations of the gills on the blood, whereas it seems intended to regulate the specific weight of fish, so that it may,

without difficulty, rise or sink in the water. In fresh water fish, this bladder contains, according to ERMAN's experiment, nitrogene gas, mixed with varying portions of oxygene gas, which is, however, never found in it in the same proportion as in atmospheric air. BIOT found, on the contrary, that in salt-water fish it contained oxygene gas, increasing in proportion as the fish was in the habit of living at a greater depth, so that in fishes, which were caught in water 1000 metres deep, the air contained from $\frac{2}{3}$ to $\frac{9}{10}$ of oxygene gas. In the bladder of such fish, the air is so compressed by the heavy column of water above, that when the fish is drawn up, the bladder swells and forces the stomach through the mouth. In one kind of fish, the *cobitis fossilis*, ERMAN observed a double respiration. In water, containing air, the fish breathed as usual, through its gills; but if the water was deprived of its portion of oxygene gas, the fish rose above the surface, drew air through its mouth and swallowed it. This air penetrated the intestines, the blood vessels of which were reddened, and when it had lost its portion of oxygene gas, the fish discharged it by the rectum.

The respiration of insects was very carefully

examined by HAUSSMAN, who found, that they absorbed oxygene gas and returned carbonic acid. He even examined the respiration of some worms, and perceived, that they also changed oxygene gas into carbonic acid. SPALLANZANI had observed the same long before, but he also believed that several of the mollusca absorbed nitrogene gas, a circumstance, which we must call in question, till it is confirmed or rejected by further chemical experiments.

I have already remarked, that that portion of the blood, which goes to the reproduction of the various parts of the body, is void of colouring matter, and penetrates the last fine ramifications of the arteries, from whence it cannot afterwards be returned, but must exude through the openings of the capillary vessels. Just at this point those parts are generated, which are to be reproduced, whereupon the remaining part is either absorbed by a particular system of vessels, which from their functions bear the name of absorbents, or discharged by means of secretions and excretions. These vessels are placed in all parts of the body with their absorbing extremities open, and take up not only the remainder of the uncoloured blood after the reproduction, but also

those parts, which have been destroyed in executing their respective functions, and they conduct from the intestines the matters prepared there for the regeneration of blood, These vessels, which are so very important, when we consider their functions, are extremely small, and for that reason very difficult to examine in an anatomical, and still more in a chemical sense; consequently we have no certain knowledge of their composition, nor of the mechanical process, whereby the humours are conveyed through them. Their own humour, the lymph, varying in its modifications according to the place, from which it has been extracted, is also but little known. We have hitherto only a single analysis of it, which however, affords us considerable information. It was instituted by EMMERT and REUSS, and the result of it was, that the lymph resembles serum in its appearance, and, when observed with a compound microscope, is perceived to be a complete chemical solution; but it thickens after some time, and becomes a coagulum, which is altogether like the fibrin of the blood. This humour consequently contains dissolved fibrin, from which we further infer, that the serum, which penetrates the reproducing capillary vessels, must also contain it, and that,

if the fibrin is here coagulated from a complete solution, the same must be the case in the blood.

It would be of the greatest consequence to animal chemistry, were we able to make a comparison between the humour in the capillary vessels before the secretion, and the same humour after it has been taken up by the absorbents. There is no doubt but that we should find differences according to the dissimilarity of what has been secreted or reproduced. Another circumstance of great moment, which is wanting in the examination of the fluid in the absorbents, is to know the nature of the decayed and useless parts, which have been absorbed by these vessels, and can only be carried off in that way. The examination of the humours in the muscles, and of the urine, gives me reason to suppose, that most matters are changed into lactic acid, phosphoric acid, and the other animal matters, which are soluble in water and spirits, and which accompany the lactates in the humours of the body, and constitute the syrupy extract, which is obtained in their analysis. If this be the case, the humours of the absorbents, after the coagulation of the albumen by boiling and evaporation of the water, must leave

a far greater quantity of the syrupy extract than the serum. That part of the humours of the capillary vessels, which is not intended to be brought back into the blood, is carried off through those organs called secretory and excretory. The fresh humours, which are formed in these organs from the constituent part of the blood, are frequently of the most different character; but, in comparing them more accurately with each other, I have found, that they all have their own characteristic constituents, among which the greater part still retain some of the characters of the albumen and the fibrin, from which they are produced. I have given them names according to the humours in which they appear, as *bilious matter, lacrymal matter, &c.* The humour, in which these characteristic constituents are found to be dissolved, contains the salts of the blood, and often its alkali, in the same quantity as in the blood. Some of the secreted humours are equally concentrated with the blood, as for instance, the *bile;* others again are more aqueous, but none are more concentrated than the blood. The secretions, or such humours, as before their discharge are intended to be used for some purpose within the body, are alkaline; the excretions, on the other

hand, or such as are to be thrown off immediately are all acid, for instance, *sweat*, *urine*, *milk*, and the free acid which they contain is lactic.

What I have hitherto remarked, constitutes the sum of our knowledge relative to the two systems, on which animal life principally depends, viz. the *nerves* and *blood vessels*. Through these, the other branches of the body execute the various operations, for which they are destined. They consequently escape our notice as to the manner in which they perform their functions; our examinations are therefore limited to what they perform.

The internal parts of the body lie closely joined to each other, leaving only small interstices, filled with a peculiar matter, which, from its structure has obtained the name of cellular texture. To form to ourselves some idea of this matter, we may consider it as an intervening medium, intended to prevent any one space from being empty. This cellular texture consequently extends itself over the whole body, and is met with in all its parts, connecting itself with the interior parts of several of them, as for instance, with the muscles. Its composition has been but imper-

fectly examined, and what we know about it is not the fruit of a direct investigation, but a collection of accidental observations, and from these we have learned, that the cellular texture, when boiled slowly, is dissolved by degrees in great measure, and that the solution, whilst cooling, coagulates to a jelly and contains glue. Several other animal matters have the same property, as for instance, the *cartilage* and the *skin*. These, however, have not the same texture, and probably not the same composition, since they cannot all be completely dissolved with the same ease. This glue does not exist as such in the animal matters, but is a product of the boiling. The erroneous idea, that glue was found within the living body, and was dissolved in its humours, has been supported by the experiment of ascertaining its presence by means of infusion of galls; but although many other animal matters are precipitated by the tanning principle, yet the precipitate with glue has this distinguishing characteristic, that it clogs together into a tough elastic mass, which, when dried, becomes hard and brittle. Such a precipitate cannot be obtained from any animal humours, except from urine, after it has previously been boiled some time with alkali, whereby the dissolved animal mat-

ters in the urine, through the effect of the alkali and boiling, probably approximate more to the nature of a glue.

The *cellular texture* contains in its small cellules a particular humour, which, though it has not been examined, we may upon good ground consider as of the same kind with that, which is met with in the cavities of the body, in blisters, dropsy, &c. In some places it contains also a half-fluid fat, the consistency of which may vary in different parts, but which, as to its chemical properties, resembles the fat oils from vegetables. In the many different modes of preparation, by which this fat is obtained for technical purposes, it partly receives foreign mixtures, and partly undergoes some less perceptible changes of composition, whereby it assumes different qualities. A right knowledge of these circumstances, although not very material to Animal Chemistry, would, however, be of great consequence in procuring better and more useful kinds of fat. Among the productions from the distillation of fat, an acid fœtid water is obtained, which CARTHEUSER considered to be a peculiar acid, which, when combined with an oil, constituted the fat. SEGNER and KNAPE also examined

this acid, and VON CRELL wrote an explicit treatise upon it, in which he examined its properties, and gave it the name of *sebacic acid.* Several Chemists afterwards proposed new methods of preparing it. THENARD shewed at last, that this acid consisted of the acetic and muriatic acids, together with an empyreumatic oil, of a highly offensive smell, which was dissolved in it. On the other hand, he found in this very oil an acid, which might be extracted by boiling it in water, and which, during the cooling of the water, precipitated itself in small light granular crystals, which he considered as a peculiar acid and called it sebacic acid. I have myself since found, in the same acid, with the exception of a few external characters, all the properties of benzoic acid, and from this I consider THENARD'S sebacic acid as benzoic acid, impregnated with the residuum of other products of distillation, which evidently impart a smell, both to the acid and to its salts, and which modify their taste.

When any part of the cellular texture is inflamed, that kind of inflammation takes place which is called *phlegmonic.* When this is suffered to suppurate, the greatest part of that which is inflamed changes itself into a peculiar humour, called pus,

which dissolves the adjacent parts, and at last, when its situation allows it, empties itself through the skin. Many chemical experiments have been made with this pus; the object of which, however, has only been to learn to distinguish it in diseases of the lungs from the expectorated mucus, that physicians might be able the better to judge of the nature of the disease; but, notwithstanding all these experiments, we have not attained any satisfactory results; however, the mucus in the trachea aud bronchia has chemical characters very distinct from those of pus, since the former is very easily dissolved by acids, and remains thus dissolved, and the latter requires the acids to be more concentrated, and may, after the solution, again be precipitated by water. The reason why the methods of examination, proposed by DARWIN, BRUGGMANS, GRASMEYER and others, have had no certain results, is this, that no correct distinction has been made between mucus and pus. They always considered as mucus, the yellow matter, which is expectorated after the crisis of the inflammation in the lungs, and where no destruction of parts had occurred. On the other hand, they considered that only as pus, which is generated in an abscess or in an open suppurating ulcer in the lungs. This

is, however, a mistake, for both are pus (originating in the coloured blood, which, during inflammation, is carried into the capillary vessels) which exudes on the membrane of the trachea, mixed with mucus, but which in the cellular texture has no other outlet, than that, which it can procure to itself by the dissolution of the adjoining parts; hence, when chemically analysed, it must be found to contain more constituent parts, than the matter produced on the mucous membrane. I am convinced, that by a proper comparative examination, the difference might be discovered, and the physician enabled, through the analysis of the expectorated matter, to determine whether it proceeds from an open ulceration, or is formed only in the mucous membrane. But, hitherto, we have no such examination. GEORGE PEARSON has indeed lately described the different varieties of the expectorated mucous matter, and fully examined the relation of the different kinds to heat, water, spirits, and acetic acid; he has also made some attempts toward their analysis, which evince both attention and diligent research; but as he could not know some of the substances, which are generally found in animal humours, and as he seemed to want the experience, which in every research is so neces-

sary for forming a correct judgement of what we see; these analytical investigations have not been productive of the information which one might have expected, if made by a Chemist who had also examined other animal fluids. However, among the results of his experiments, there is much that deserves to be noticed. In the mucous matter, which is expectorated in long catarrhal coughs, he found, when diluted with water and observed with a good miscrocope, heaps of small globular bodies, which were carried to and fro, as it were, with a spontaneous motion. These globules were somewhat larger than the globules of the colouring matter in the blood, and could not be destroyed, either by trituration or boiling, nor by drying or re-dissolving, neither by coagulation with mineral acids, alcohol, æther, alum, or tanning principle, nor by an addition of so small a quantity of caustic alkali, that the humour still kept itself turbid, and even a commencing putrefaction did not break them down; but they were destroyed by concentrated sulphuric acid, and by employing so much caustic alkali, as to render the solution clear, or by heating the dried matter until it began to be charred. He found these globules also in expectorated pus in a decided case of consumption; but his sup-

position, that they consist of an organized carbon, seems not a fortunate one. He also found, that the thinner these expectorations were, the more salts they contained, and the sooner they attracted moisture from the air; but this latter circumstance was probably only a consequence of the smaller quantity of fixed matters, whereby those constituent parts, which could attract humidity, were surrounded. When, during his experiments, he found the syrupy extract, which, in the analysis of blood, flesh, milk, and urine, I have shewn to be a composition of muriate of soda, alkaline lactate, and some peculiar animal matters, he considered it as an animal oxid, which possessed the quality common to acids, of neutralizing a certain portion of alkali so completely, that it was no longer discovered by the re-agents. In the ashes of the burnt pus, he found, besides the common constituents, also silica and oxid of iron.

The loss which the humours of the body daily suffer by excretions, as also that which the blood must experience, from the reproduction of parts, is day by day compensated by the food which is eaten. Chemistry has been much occupied in explaining the processes, which take place in the

stomach, and in the bowels; and here, as every where else, it has been obliged to stop short, where the nervous system has begun to determine the chemical state of these processes.

Through BICHAT'S excellent investigation into the membranes of the body, we have acquired better information relative to the vessels, in which the processes of digestion are performed, and have also been enabled to attain a more precise chemical knowledge of the composition of the membranes of vessels. The whole intestinal canal, and all the reservoirs, with their excretory ducts, which have any connexion with it, are coated on the inside with a mucous membrane; so called, because it keeps itself constantly covered with a mucus, by which it is defended against any effect from those bodies, with which it comes in contact. The chemical composition of this mucous membrane has been satisfactorily examined by BICHAT: its principal character is insolubility in boiling water—we derive no glue from it like that from cellular and serous membranes (M. serosæ); and of all parts of the body, the brain excepted, it is soonest destroyed by being macerated in cold water, or treated with acids. The mucus, with which the membrane is covered, is al-

most every where alike as to its external character, but in other respects, it varies very much in its chemical properties, according to the different nature of the substances with which it is destined to come in contact. I have, myself, found in an experiment I made on the mucus, that it has certain varying chemical characters in the nose, in the trachea, in the gall bladder, in the urinary bladder, and in the bowels, without which it could not answer its destined purposes. The mucus, in the nature of its composition, is not a solution; but contains a solid body, which has the property of swelling in water and becoming a tough half-liquid mass, which, however, is not dissolved if more water is added, and which may be deprived of its water, by placing it on blotting paper, and thereby rendered more dense. The humour by which the mucus is penetrated is nothing else than serum, which, however, has lost almost its whole portion of albumen, retaining only the other constituent parts. The peculiar substance, which forms the mass of the mucus, for instance, in the nose, is soluble both in acids and in alkalies, although somewhat more slowly in the latter; but, on the contrary, it is very easily dissolved, in the gall-bladder, by means of alkali; but is completely precipitated by acids. By this property, the mucus

dissolved in the bile, is precipitated by the acid in the chyme, when the bile is decompounded during digestion. The mucus of the gall bladder, if it had the properties of that in the nose and trachea, would remain in solution in the chyle, and, thereby, be less useful in the animal œconomy. Several Authors mention an animal mucus, as a substance distinct from the mucus of the membranes, and supposed to be found in the fluids of the animal body. I can only say, that in all my experiments, I have never found any matter, to which this name could be applied, or which agreed, satisfactorily, with the character of those substances, which HATCHETT, BOSTOCK, JORDAN, and others, have designated by that name. It appears probable to me, that with most of them, mucus has been a common name for matters, which could not be distinctly specified. The celebrated FOURCROY, a little before his death, left us a treatise on mucus, in the sense in which I have here used the term; but this treatise was not occasioned by any direct investigation, but was a mere result of scattered observations, whereby he fell into an error (to him not very uncommon) of drawing general and extensive conclusions from very uncertain, and sometimes incorrect observations; and with these ingenious and interesting

representations, the credulous and inexperienced reader has been deceived. In this treatise he generalized the idea of mucus so far, that according to his statement, the epidermis, nails, silk, hair, and other substances of the most different chemical and physiological characters, were nothing else than hardened mucus.

The intestinal canal, besides of the mucous membrane, is surrounded by a dense cellular and a muscular membrane, corresponding in their chemical properties, the former with the cellular texture, and the latter with the muscles. On the whole of its outside, from the throat to the rectum, it is enveloped by the serous membrane of the abdomen. These serous membranes, which are found in all the cavities of the body (to conclude from the imperfect chemical experiments that have been made on them) consist of the same fundamental mass as the cellular texture. They are called serous, because a serous fluid constantly moistens them, and prevents their adhesion. This fluid is very considerable as to its quantity; it has been chiefly collected in dropsies, and never examined in its healthy state; we have, however, every reason to suppose, that its composition on these occasions is not altered. According to some

experiments, which I have myself made, this fluid consists of serum, which has lost the greater part of its albumen, of which, however, so much remains, that the fluid, during boiling, becomes in a very small degree coagulated. During its evaporation common salt is crystallized from it, and the usual brown extract is deposited between the crystals, containing alkali, lactate of alkali, and the extractive animal matters, which commonly accompany them.

The *Fluids*, which some way or other contribute to digestion, are the *saliva*, *the gastric juice*, *the bile*, *the fluid of the pancreas*, and that of *the intestines*, (succus intestinalis). FOURCROY and VAUQUELIN have left us an examination of the saliva, and it has since been analysed by BOSTOCK, and lastly, I have myself endeavoured to ascertain its composition. I found it to be one of the most aqueous fluids of the body. It contains a suspended white mucous matter, which is easily separated by the solution of the saliva in water, and which is soon dissolved by alkali, but not by acids. I have reason to believe, that, to a certain extent, at least, it originates in the mucous membrane of the salivary ducts, and

the inside of the mouth. The other part of the saliva contains, besides the usual salts of the serum, a peculiar matter, remarkable in this respect, that it cannot be coagulated either by boiling, or by the tanning principle, or the subacetate of lead. With water it resolves itself into a liquid frothy solution, although it should be observed, that the saliva derives its property of forming threads only from the mucus, which is mixed with it. The viscidity of the saliva has been considered only as intended to mix small particles of air with the substance, which is chewed. This, however, I consider as a mistake. It may probably have the mechanic purpose of preparing a slippery and coherent mass from the chewed food, in order to promote its being more easily swallowed; but it is not at all decided how far it may contribute to the solution of the food. When part of the mucus of the saliva remains on the teeth, it thickens, gets coloured, and forms what is called *tartar;* and I have found this to be of a two-fold kind: when just settled it is clearly nothing else than the darkened mucus; but during the destruction of the mucus we insensibly perceive phosphate of lime on the enamel of the tooth, which sometimes is increased to a crust of

the thickness of $\frac{1}{4}$ to $\frac{1}{2}$ a line. This contains, besides the phosphate, about $\frac{1}{5}$ its weight of mucus, which has been dried up in the earthy mass.

Several Natural Philosophers, both ancient and modern, such as STEVENS, REAUMUR, SPALLANZANI, SCOPOLI, BRUGNATELLI, CARMINATI, VAUQUELIN, &c. have endeavoured to investigate the composition of the gastric juice; but as animal fluids in general had been little examined, and the greater part of these philosophers were not much acquainted with Animal Chemistry, all their analytical attempts were productive of no satisfactory results, since they could not compare the substances that were found in the gastric juice with the constituent parts of other fluids. VAUQUELIN always found phosphoric acid in the gastric juice of herbivorous animals; whilst on the other hand, that of man and carnivorous animals, had seldom any visible traces of acid or free alkali. One of the most remarkable chemical properties of the gastric juice is its power of dissolving the nutriments, which the animal consumes, and of coagulating milk and albuminous substances. The latter requires so small a quantity of the coagulating matter, that YOUNG found, that if the interior mucous mem-

brane of the stomach (which after the death of the animal retains in its vessels part of the gastric juice, which in the moment of death was about to be secreted) be first washed with water and then with a weak alkaline solution, the water with which it is macerated, has, even after this operation, the power of coagulating milk and serum. It is not yet ascertained what substance it is, that gives these distinguishing qualities to the gastric juice. In the mean time, it is asserted by some, that flesh, wrapped in a fine piece of linen, and afterwards placed in a situation, where it is penetrated by the matter of perspiration, as under the arms, between the toes, &c. becomes dissolved in the same manner as by the gastric juice.

The *Fluid of the pancreas* has never been chemically examined; it is supposed, however, from the structure of the gland, which resembles that of the salivary glands, that it is analogous to the saliva in its composition. The *bile*, on the contrary, has often been an object of chemical examination. Boerhave, Bianchi, Verheyen, Hoffman, Drelincourt, Hartman, Marheer, Barchhusen, Schroeder, and others of the old school, have occupied themselves a good

deal with it; but CADET gave us the first tolerably accurate analysis on the subject, and some years after him VAN BOCHAUT also engaged in the investigation. Those who have since laboured in this field are MACLURG, FOURCROY, POWELL, and very lately THENARD. The old experimenters all agreed in considering the bile as a kind of soap, composed of caustic soda, and a peculiar green bitter resin, which could be precipitated by acids, and supposing that the saponaceous substance was mixed with the bile in a certain proportion. It was also considered as containing a portion of albumen, which could be separated by alcohol; THENARD, however, shewed that the bile contained, besides this resin, a peculiar bitter-sweet matter, in form of an extract, to which, from its taste, he gave the name *picromel,* and which, together with the alkali, contributed to hold the resin dissolved. Some circumstances in THENARD'S experiments, which appeared rather improbable, made me also undertake an analysis of the bile, the result of which was the discovery, that none of my predecessors had properly ascertained the composition of it: I found that it contains no resin whatever; that it has the same proportion of alkali and salts as the blood; and that it contains a pe-

culiar matter of a bitter and afterwards somewhat sweet taste, which possesses characters in common with the fibrin, the colouring matter, and the albumen of the blood, from which it is formed in the liver. I found, also, that with mineral acids it forms a peculiar substance, not easily soluble in water. With an excess of acid it is completely precipitated, and has all the characters of a resin, viz. is soluble in alcohol, melts in heat, forms a composition like a plaster with the oxid of lead, &c. A smaller quantity of acid, on the contrary, produces a more soluble compound. The resin, which is precipitated with sulphuric acid, may be restored to its original properties by being treated with carbonate of barita, and then produces a solution altogether like bile. This peculiar matter agrees also with the albumen and the fibrin of the blood in this respect, that it cannot be precipitated by acetic acid. In different animals, as also under different circumstances in the same species, it has a different tendency to form almost insoluble combinations with the acids; and from the experiments that I have had an opportunity of making, I drew the conclusion, that its remaining long in the gall-bladder, increases its tendency to form resin with the acids. All my predecessors have allotted a

portion of albumen to the bile; but as the matter which they have here called albumen is precipitated from the bile by acetic acid, and cannot be dissolved in an excess of acid, it must, of course, be something else. In an examination of the mucus from different parts of the mucous membranes, I demonstrated, that this matter is nothing else, than a part of the mucus of the gall-bladder, which has become dissolved in the bile, and made it what physicians term, more *involved.* The bile, however, contains it in very small quantities; for when this fluid is very thick it does not afford a perceptibly greater residuum after evaporation, than when it is quite thin.

This is all that we know of those fluids, which have any thing to do with digestion; and although this process is more independent of the immediate influence of the nervous system than most others, it is far from being sufficiently understood. It was for a long time believed that the office of the stomach, in digestion, was nothing else than a mechanical effect of its membranes, in triturating the food. The experiments, however, of STEVENS, REAUMUR, and SPALLANZANI, have proved the incorrectness of this supposition. They caused animals to swallow tubes, and balls of metal, per-

forated with small holes, in order to admit the fluids of the stomach; in these they put nutritious substances, and they found, that this food, after a-while, was equally changed into chyme with that in the other part of the stomach. A mechanical effect of the coats, could not therefore be the cause of digestion. Fermentation was next resorted to; but when it was found, that meat, in close balls of metal, was not changed in the stomach, and that small bones, which had been swallowed, were dissolved and had lost their cohesion, the theory of fermentation was relinquished; and that opinion was adopted, which is still considered as most probable, that a peculiar fluid, *the gastric juice*, is secreted in the stomach, which possesses the property of dissolving the nutritious substances that enter that organ, and by this dissolution preparing them for the further changes, which are to take place in the duodenum. In consequence of these ideas, SPALLANZANI instituted numerous experiments to ascertain the nature and property of the gastric juice, the repetition of which would, at present, be productive of far greater advantages than could have been expected in his time. EAGLESFIELD SMITH has endeavoured to prove, by experiments on frogs, that the bile, and not the gastric juice, was the means of solution,

since the food in the stomach of these animals was always found unchanged if the biliary duct was tied up,; but if he administered bile to them, or untied the ligature, digestion was very soon performed. But even should these observations and conclusions be found correct, as they regard the amphibia, the case is still different in the mammalia; for we have many instances of perfectly prepared chyme being thrown up, in which no sign of bile has been perceived, and when bile at any time is met with in the human stomach, it is always, according to well-established experience, a certain sign of disease.

Everard Home has lately endeavoured to prove, that the stomach, during the time of digestion, is kept divided into two parts by its muscular fibres. He supposed that the portion nearer to the throat is destined to contain liquid nutriments, and that in the more remote the solid nutritious substances are to be dissolved. The object of this division of the stomach, during the process of digestion, which certainly seems not very probable, he supposes to be, to convey the greater part of those fluids through an unknown channel from the stomach into the spleen, that they may mix with the blood more speedily, than by the

course of the absorbent vessels from the intestinal canal. This passage, which could not be altogether unexpected by those who were acquainted with the minute investigations, which were formerly instituted to discover a direct way from the stomach to the kidneys, HOME has endeavoured to support, by experiments, which, in case they have been correctly observed and confirmed by other experiments, do certainly prove the point most completely. He found, for instance, that when a person takes a dilute infusion of rhubarb, the colouring matter of the rhubarb shows itself about twenty minutes after, in the urine, but soon disappears again, and does not return till after several hours, when the remainder of the infusion of rhubarb has passed the thoracic duct. When an animal is made to take infusion of rhubarb, and is killed shortly after, the infusion is discoverable in the blood of the spleen, but not, for instance, in that of the liver, &c. He found, that the spleen contained a number of small cellules filled with a colourless fluid during the period of digestion; but which, at other times, were collapsed and almost imperceptible. The same observations, relative to the structure of the spleen, have also been made by other respectable ana-

tomists before HOME. In this manner the chyme always acquires a determined consistence, since the superfluous fluids, which have been swallowed, are emptied without being mixed with it; while the chyme is conducted through the pylorus into the duodenum, where it comes in contact with the bile. The change, which the bile here undergoes, is unknown, both as to its nature and its purpose. That it is really decomposed, we may learn from this circumstance, that it is no longer met with in the contents of the intestines as bile; but instead of it, they contain the peculiar matter of the bile, changed into a kind of yellow or greenish adipocerous fat, which gives to these contents their darker colour. It was thought that no chyle could be formed without the assistance of the bile, and though it cannot be denied, that the bile may be indispensable for the formation of perfect chyle, yet we have instances of persons, in whom, during the course of a chronic jaundice, the flow of the bile has been obstructed for two or three weeks together, and yet they have not died for want of nutriment. After the bile and the pancreatic juice have mixed themselves with the chyme, we find it distinctly divided into a kind of white emulsion, which,

from its colour, is called lacteal fluid or chyle, and another light yellow insoluble part, which is destined to form an excretion. This insoluble part consists of such parts of the food, as cannot be dissolved by the gastric juice, and of the bilious matter, which is precipitated in the form of a coloured adipocere, and probably in this state is united to certain parts of the chyme, which, by their affinity, have determined its precipitation. Both are now blended together. The absorbents, which arise every where in the villous surface of the mucous membrane, absorb the dissolved part and leave the undissolved; but as the mass, by means of this absorption, would at last become very dry, before the whole of the chyle had been taken up, a thin liquid humour is secreted on the inside of the intestines, which unites itself with it, dissolves the chyle, and is afterwards absorbed by the succeeding portions of the intestines, so that when their contents arrive at the sphincter, there is often none of the chyle left in them. The nature of this mass, after excretion, has been very little examined by former Chemists; and the experiments which Vauquelin and Sage in later times have left us, cannot be considered as complete,

EINHOF and THAER instituted a very minute examination of the excrements of cattle, and I have since examined those of man in a more physiological point of view than my predecessors. These contain about ¾ of their weight of fluid, in which is found, besides the small portion of yet undecompounded bile, a solution of the usual salts of the serum, a certain quantity of phosphate of magnesia, and a peculiar animal matter in form of an extract. The solid mass consists of what is undissolved in the stomach, or precipitated in the duodenum, and of the mucus of the intestines, which is thoroughly diffused through the whole mass.

VAUQUELIN instituted a comparative experiment on the quantity of earths a hen consumed in the oats on which it was fed, and the quantity produced in the eggs and excrements, whereby it appeared, that the quantity of phosphate of lime in the latter was twice as much as in the former, besides a quantity of carbonate of lime, found in the oats on which it was fed, and that the quantity of silica was somewhat, though not considerably, diminished in the excretions of the hen. These experiments, which, by the bye, have been

made very intricate by incorrect decimal cyphers and erroneous calculations, seem to prove, what experiments with vegetables further confirm, that these earths must be capable of being composed and decomposed, as occasion requires, by the processes of organic chemistry.

The *Chyle*, such as it occurs in the smaller intestines and in the first branches of the absorbent vessels, is not distinctly known. From its colour and its property of coagulating, whilst boiling, it was compared to milk, and it was long supposed that its principal constituent parts consisted of sugar of milk. This error is, however, now corrected. As soon as the chyle has come into the thoracic duct, its milky colour gets more and more faint in proportion as it is diluted with lymph from other absorbing vessels. In general it is a mixture of yellow and grey, is coagulated in the air, and the coagulum assumes, by degrees, a red colour. We have, consequently, reason to consider the suspended white and grey matter in the chyle as a colouring matter not yet completed, and which wants the contact of the air, in order to be perfected. The experiment on the chyle, by HALLÉ, EMMERT, and REUSS, all agree

in this, that chyle, except in the colour, resembles blood, but is a much more dilute fluid. After FOURCROY had stated that the colouring matter of the blood was a combination of red subphosphate of iron with albumen, it followed, that the white colouring matter in the chyle must be albumen united with a neutral phosphate of protoxid of iron, which at its entrance into the blood acquired an excess of base by the alkali of the blood, and which was changed in the lungs from protoxid to peroxid; but as no such ferruginous salt can be discovered in the colouring matter of the blood, all this seducing supposition falls to the ground.

A summary idea of the formation of the chyle by digestion, may thus be expressed in a few words: the alimentary matters are accurately triturated in the mouth, received into the stomach, and there converted by the gastric juice into a uniform fluid, which is precipitated in the duodenum by the bile. The solution is filtered in the intestines by means of the absorbents, and the precipitated matter is washed by the intestinal fluid, which is again absorbed, in the same manner as precipitates are edulcorated in our common filtering apparatuses, after which the washed mass is evacuated.

The composition of the mass of the *liver* and the *spleen* has not been examined. It has only been observed, that the liver, when putrifying, so far partakes of the property of the bile, as to be easily changed, under certain circumstances, into a substance resembling adipocere.

The texture and constituent parts of the *bones* have only of late been accurately ascertained, although it was discovered, very early, that they contained an animal combustible part, and an unorganic earthy part, which, as it did not resemble any other earth then known, was called bone-earth, or animal earth. PAPIN, HERISSANT, LESSONE, and above all, HALLER, have proved, that the combustible part was cartilage, which might be dissolved into a glue by boiling. The nature and composition of bone-earth was discovered by a gentleman, whom we have the happiness of seeing this day amongst us, I mean our colleague G. GAHN. This, as well as several other of his discoveries, have been ascribed to SCHEELE and BERGMAN, because these gentlemen, to whom we were accustomed to ascribe so much, were the first to communicate them to the public, though with an unintentional omission of his name. It was enough for this modest natural

philosopher, that truth was made known. He did not claim the honour of the discovery, since it was indifferent to him, if his former Instructor or his Friend were supposed to have found out what he, himself, had discovered; but posterity will not forget to make amends for the neglect or omission of contemporaries. SCHEELE, in his treatise on the fluor spar and its acid, stated that bone-earth, "as was proved by a late discovery," consisted of phosphoric acid and lime, and this expression gave rise to the mistake, for the discovery was made by GAHN.

FOURCROY, a long time afterwards, proved, that the bones of herbivorous animals contained phosphate of magnesia, which he, in vain, sought after in those of man; and at last MORICHINI, an Italian, discovered, that ivory, as well as the enamel of the teeth, contained fluate of lime. Besides the constituent parts which were already known, I have, by minute analysis, detected in human bone, fluate of lime and phosphate of magnesia; and I have proved, that the sulphate of lime, which after combustion is found in them, does not belong to them in their living state. The cartilage of the bones I found to be about $\frac{1}{3}$ part of their mass. It was somewhat less in the

teeth, and entirely wanting in the enamel of the teeth. FOURCROY and VAUQUELIN had found 27 p. c. combustible matter in the enamel. PEPYS only 16 p. c. but I could not find 2 p. c. of it. MERAT-GUILLOT has instituted a comparative investigation on the bones of several animals; but it has produced very incorrect results. While I found, for instance, in the human bones and those of the bullock, the same constituent parts, and nearly the same proportions, he found 93 p. c. phosphate of lime in the latter, and 67 in the former. The cartilage, which is the animal constituent part of the bones, is so closely united with the bone-earth, that they constitute a common *whole*, of a texture, which withstands considerable impressions, and of a chemical composition, which, when the bones have not been moistened, has preserved itself unaltered for several centuries. In later times we have known how to profit by PAPIN'S excellent discovery, of extracting from bones a good and salutary nutriment, which in his days was disregarded. PAPIN had shewn to Charles II. of England, that it was possible to extract a jelly from the bones, and had engaged within 24 hours with eleven pounds of charcoal to prepare 150 pounds of a jelly, which he recommended to be used in workhouses and in

hospitals. The King was ready to give this project the attention it deserved, when, as he was one day going to dinner, he found petitions fastened to the necks of his dogs, the import of which was, that he should not deprive them of a food, which they had long considered as their property. The jest was smiled at, and PAPIN'S discovery was lost for the age. L. PROUST recalled the public attention to it, and proposed to the Spanish government to use the bones for the soldiers, and in public institutions for the poor, and his proposal was adopted. The French government, observing the progress of their neighbours, ordered this matter to be investigated in France, where it was soon adopted, and afterwards spread itself throughout Europe. Several learned men have occupied themselves with experiments for extracting the cartilage of bones completely, and it has almost generally been found impracticable, except by using PAPIN'S boiler, on which such improvements have been made of late, that it may now be used without any danger. Some have gone so far in their zeal for the bone soup, that they have considered bones as more nutritious,and consequently of more value, than an equal weight of meat. This, however, is not cor-

rect, for the reasons, which I have stated in the analysis of muscular fibre.

On the composition of the bones, according to the different age of the individual, we have no satisfactory experiments; neither do we know the general differences of the bones of different classes of animals. HATCHETT has examined the integuments or *coverings* of the testacea, which we justly consider as the bones of those animals, and he found them to be composed of a peculiar animal matter, the nature and chemical properties of which he did not minutely determine, and of carbonate and phosphate of lime. While the bones of the mammalia consist of phosphate of lime with but little carbonate, these shells, on the contrary, consist of carbonate of lime with a few p. c. of the phosphate.

Marrow, or the fat, which is contained in the long bones, appears, by my experiments, to be similar to fat in other parts of the body; and the different properties it acquires by being boiled whilst included in the bone, belong entirely to the fluids, contained in the vessels of the proper membrane of the marrow, and which it loses altogether, when the fat is melted from its cells.

The composition of cartilage is exactly the same as that of the cartilage contained in bones. It is dissolved by being boiled in water, and is changed to a glue, its veins and nerves remaining undissolved.

The *Sinovia*, contained in the joints between the bones, has been examined by MARGUERON, a Frenchman. It seems to be almost completely serum, which has retained its portion of fibrin, for it coagulates in atmospheric air: this coagulum, however, is not coloured, and the rest is entirely similar to the fluid in coagulated blood. When MARGEURON stated the properties of fibrin in this humour to be 12 p, c. he weighed it, most probably, in that moist state in which it is found on coagulation. This fibrin has not altogether the same properties as the fibrin of the blood, but from his analysis no very precise knowledge of the difference can be derived. FOURCROY seemed to think that it was uric acid, for which opinion there appears to be no other reason, than that gouty concretions, which consist of urate of soda, are sometimes formed near the joints, and obstruct their motion.

The *Muscles*, (or what is generally termed flesh) have been less examined than other animal

matters. GEOFFROY endeavoured to ascertain how much could be extracted from them by water, and the different quantity of nutritious substances, which might he obtained from the flesh of different animals. THOUVENEL next gave us an analysis of flesh, which he considered as composed of the muscular fibres, properly so called, and a peculiar extract, soluble in alcohol and water. I have since found, that flesh contains near ¾ of its weight of fluid; that this fluid contains a free acid, and that the extract, which THOUVENEL described, is the same acid syrupy mass, which is met with in milk and urine, and which consists of lactic acid, lactate of alkali, salt, and the animal matter, which is united with these salts in the form of an extract. I have endeavoured to prove that this extract is not a constituent part of the flesh, but that it has belonged to the absorbents, and that it consists principally of the decayed particles, which have been either absorbed by them, or were ready to be absorbed when life was extinguished. The fluids of the flesh abound much more in this syrupy extract, and contain more phosphate of soda, than the blood; and from this, I have concluded, that those matters, which are formed by the decay of the parts, are absorbed and introduced into the blood, in order to be discharged with the urine, in which

they are again discoverable in a considerable quantity. The solid living muscular fibre is, as anatomy will shew, interwoven with the cellular texture, and furnished, even in its innermost parts with veins and nerves. This fibre has the same qualities with the fibrin of the blood; it is soluble in acetic acid, except the greater part of the cellular texture, together with the vessels and nerves whereby it is supported. The fibrin of the flesh undergoes the same change by boiling as that of the blood; it thereby becomes insoluble in acetic acid, and leaves to the water, with which it has been boiled, a constituent part, which has a strong and pleasant taste of flesh and cannot be gelatinised. When this is dissolved, together with the cellular texture, and mixes itself with the uncoagulated part of the humours of the flesh, it forms what we call broth, the strength and taste of which depends not only on the dissolved and glutinised cellular texture, but also on the fibrin, the taste of which it retains. The difference between the tasteless bone-soup and the broth, has formerly been ascribed to the extractive substance; but this cannot be correct, since we know that flesh, the humours of which have been extracted, gives a very palatable and nourishing, though at the same time, colourless soup.

Various experiments have been made to discover the intimate nature of the motion of the muscles. CARLISLE has endeavoured to prove, by several very ingenious methods, that a muscle, while it contracts, increases in weight and volume, and is consequently penetrated by an increased quantity of fluids. The most probable account of the interior mechanism by which the motion of the muscle is produced, seems to be, that the fibres must twist themselves round more distended vessels, whereby the muscle in a particular place becomes shorter and thicker. It is necessary that nature, by the mechanism of this internal action, should endeavour to regain what it loses by that of the external, in which every thing is conducted according to principles, altogether contrary to those, by which we are generally governed in mechanics. The muscle, by means of a small contraction, makes an extended motion, and consequently exercises its power near the fulcrum of the bone, whilst the counterpoise is on the other remoter end of the bone. Nature seems here to be lavish in the power it bestows, since every muscle must make a far greater effort, than would be necessary under opposite circumstances; this expenditure of power must, however, certainly be balanced by the methods employed to shorten the smallest

fibres in the acting muscle. I have often determined to examine, with a compound microscope, the contraction of a naked muscle in a living animal, being convinced that by that means something might be obtained toward a nearer explanation of this highly interesting process of animal mechanics; but I have always been deterred by an insurmountable aversion to see a wounded animal suffer under the hand of an experimenter, much as I at the same time value the important physiological truths, which have been discovered in this way.

Sinews, (tendons and aponeuroses) are parts of the muscles, by which they are fixed to remote bones, or to circumscribed points of attachment. They have a mechanically strong composition, and are formed from the same fundamental mass as cellular texture and cartilage, from which they seem to differ only in their structure. They are softened by degrees by boiling, and dissolved into a glue, so that at last, the veins only remain undissolved.

Of *the Membranes of the Eye,* and the humours which are contained in them, Chemistry has had to record little else, than the observations, which have been accidentally made by Anatomists.

Chenevix, not long ago, gave us an analysis of the humours of the eye, and I have since examined their composition, together with that of the membranes.

The *Sclerotica*, which surrounds the eye on the outside, has the same component parts as the tendons: it dissolves by continued boiling in water, and the solution, whilst cooling, hardens into a jelly. The *Choroidea*, spread over its internal surface, is a similar compound, and in like manner dissolves in boiling, with the exception only of its numerous veins, and the black colouring matter, or pigmentum nigrum, with which it is covered. The black substance is indissoluble in hot or cold water, and also in acids. It is soluble, however, in caustic alkali, and the precipitate by an acid is somewhat paler. It burns like a vegetable substance, and leaves the same ferrugineous ashes as the colouring matter of the blood, from which it has, in all probability, been formed and secreted, the veins of the choroidea transmitting only the colourless parts into the substance of the eye. The *Cornea*, also, consists of a mass, which by boiling, is converted into a glue. The *Iris*, on the contrary, has all the chemical characters of a muscle, and its constituent parts

are the same as those of muscular fibre. As its operation also resembles that of these organs, it seems more than probable, that it ought to be considered as one of the muscles of the body, and in man, as one of those, which are not under the control of the will; although, on the other hand, its motion in several birds (for instance, the genera strix and psitacus,) seems to depend on the will.

The humours which fill the globe of the eye are three, viz. the *aqueous humour,* the *crystaline lens,* and the *vitreous humour.* The first and the last of these humours are very thin, and agree in their composition altogether with the fluid of the serous membranes; but they are distinguished from them in this, that they are completely void of colour, and the aqueous humour seems to contain a smaller quantity of albumen. The crystaline lens, on the contrary, is very remarkable, both as to its texture and its composition. Its density is least of all at the surface, but continues to increase towards the interior; and CHENEVIX found, by removing the coats or layers successively, that its specific gravity increased, in proportion as he approached the centre, When the process had been performed on a lens weighing 30 grains, with a specific gravity of 1,076,

until only 6 grains remained, the specific gravity of this internal nucleus proved 1,194. CHENEVIX, and before him FOURCROY, state, that the lens consists of albumen and glue; and yet it contains neither the one nor the other: it is almost completely soluble in water, and the solution is coagulated on boiling; but the coagulated mass does not resemble albumen, it is gritty and opaque, exactly like the colouring matter of the blood, which it resembles also in being easily dissolved in acetic acid, after the coagulation. The coagulum is as white as snow, and leaves after combustion a small quantity of ferrugineous ashes. As far as we can discover, it differs only from the colouring matter of the blood, by its want of colour. It is not improbable, that the colouring matter is divided into the colouring part strictly so called, which is deposited in the choroidea, and the uncoloured albuminous fundamental substance, which passes on and forms the crystalline lens; although they do not receive their blood from the same, but separate branches of one and the same artery that is, the ophthalmic. Some attempts which have been made to change a dissolved lens into colouring matter, by adding to it phosphate of iron, in various modifications, have not been more successful than

similar experiments with serum. The crystalline lens forms the limit between the humours and solid animal matter. It contains little more than half its weight of water, and differs from other secreted fluids in this, that it is less aqueous than blood. It produces, when analysed, a portion of acidulous extract, in which it resembles animal matter, divested of its alkali. REIL had found, that when the crystalline lens is treated with dilute nitric acid, it is converted into a yellow fibrous mass, like raw silk, its fibres diverging from the centre towards the surface, in a certain regular order. From this he concluded, that the lens was a muscle, whose fibres were only rendered visible by this treatment; but, although the interior construction of the lens is as yet too little known, to enable us to explain the mechanism of the phenomenon, still it is clear, from the solubility of the mass in water, and from its manner of coagulation, that the lens cannot possess the properties of a muscle.

The *Tears* have been examined by FOURCROY and VAUQUELIN—They resemble very much the humours from serous membranes, and the humours of the eye, but with this difference,

that, instead of albumen, they contain a peculiar matter in solution, which is not coagulated by the heat of boiling water or by acids, but which, during a slow evaporation in atmospheric air, is changed into an insoluble mucus, like the mucus of the nose, already mentioned. If this peculiar matter, which in my Treatise on Animal Chemistry, I have called *tårämne,* or a peculiar matter of the tears, possesses the characters, which the French Chemists have ascribed to it, it does indeed deserve to be fully investigated. Should their statement be confirmed, the mucus of the nostrils must then, like the tears, be secreted in a thin liquid state, and be converted during respiration, by the agency of the air, into mucus. This would presuppose an essential difference between the formation of the mucus in the nose, and in those places, where it is not in contact with the air, and must be directly secreted as a complete mucus.

The *Cerumen of the Ear* has been examined by VAUQUELIN. When observed after having been long detained in the meatus externus of the ear, it is dried up, and consists of a peculiar fat body resembling oil, which assumes the form of an emulsion by its combination with albuminous

matter. In the first moment of secretion it forms a thin yellowish milk, which thickens by degrees as it loses its water.

Respecting the *Skin,* and *the parts* connected with it, our chemical information is hitherto very imperfect. What we know is chiefly obtained from accidental observations in tanning and glue making. The skin (or hide) changes by long boiling into a glue, which is better and more tenacious in proportion as the skin is more difficult of solution. Tanning consists in combining the unchanged mass of the skin with tanning matter from vegetables. This tanning matter, extracted from various vegetables, often differs as much in its nature, as the hides of different animals; and from this circumstance, the results will vary according to the vegetable, which has been used in the tanning process. Skin requires a certain preparation previous to tanning. The object of which is, by means of a liquid, to penetrate and widen all the cells of the hide, in order to open a sufficient space for the reception of tanning matter. Weak alkalies have a better effect here than water, and acids succeed even better than alkalis. Vegetable acids are prefered to mineral. The quantity of acid in the water, with which

the hide is soaked, may be very small, and yet produce a considerable effect. The principle thing to be observed in tanning is, that it be done slowly, week infusions only being used in the beginning of the process. The more slowly the process is conducted, the better the leather becomes; whilst, by a hasty process, the outer part of the hide is supersaturated with tanning matter, and the avenues to the interior parts are closed; hence a hard, brittle, and thin leather is produced.

The *Rete Mucosum*, or net of Malpighi, which constitutes the secretory organ of the skin, and in which its nerves and absorbents are dispersed, has not been examined. We only know, that its black colour, observable in the skin of negroes, may be bleached for some days by the application of oxy-muriatic acid. The texture of the *Epidermis* is very peculiar: It is not dissolved even by long boiling in water, but it is dissolved both by caustic alkalies and acids, and, in most of its chemical properties, resembles the hair and the nails. The greasy substance, with which it is covered, has not been examined on the human skin; but VAUQUELIN has made an experiment on that, which adheres to the wool in sheep, and

has found it to contain, besides carbonate of alkali, albumen and the acetates of lime and potash, a peculiar saponaceous combination of potash, lime and a resinous oily substance. He ascribes the fat of the wool and the hide to a part of the oil, which has been separated on the combination of the alkali with carbonic acid from the air. There appears, however, to be a contradiction in supposing, that the greasy humour should at once contain carbonated alkali and lime in solution. VAUQUELIN and BUNIVA found a greasy matter, resembling cheese, on the skin of the fœtus, whereby its epidermis in the womb was defended from any effect of the liquor amnii.

The Skin is an organ of secretion, which is distinguished from all others by presenting an extended surface, from which the greatest part of the secreted matter must be discharged by means of evaporation. In former times much pains were taken to examine the quantity of transpiration. SANCTORIUS continued his experiments for thirty years; and after him many others have laboured in the same field, as DODART, KEIL, ROBERTSON, RYE, LINING, and at last also LAVOISIER and SEGUIN in France, and CRUIKSHANK in England. But all their experiments

went only to prove, how much moisture was perspired, without affording any information as to the quality and nature of the perspired matter. According to the mean of these experiments 4½ pounds per day, were perspired from the skin.—The secretion was most abundant during the time of digestion, and least of all, immediately after meals. CRUIKSHANK having confined his hand in a glass, collected the water which transpired, and found it to be very pure. In the air, a portion of carbonic acid was found, which in all probability had been formed by the operation of the air on the transpired matter, as the volume of the air had not increased. As he obtained more water in a low temperature than in a high one, he concluded, that the transpiration of the skin was greater in a low temperature; but it seems that he did not recollect, that the more water was condensed on the glass by cold, the greater became the power of the inclosed air to receive evaporating water, when it was warmed again by the hand. THENARD collected the matter of transpiration in flannel shirts, and found, that, after extracting it with water, and then evaporating that water, it produced an acid, saline, syrupy extract, the free acid of which he considered as acetic, in consequence of an experiment

in the course of his examination of the lactic acid, which had determined him to declare this acid to be acetic.

The matter of transpiration is always acid, and reddens litmus paper very distinctly. I collected some drops of sweat on a watch glass, and suffered them to dry, when they presented crystals of muriate of soda, and also visible marks of the usual acid extract of the secretions; on the addition of water, they left an insoluble residuum, which, when strongly heated, smelt like burnt albumen. In the mean time, much remains to be known on the subject of transpiration, before we can discover the various matters secreted by it from the body, and the different modifications to which it is subject, according to different circumstances.

The *Nails*, which are a continuation of the skin, are of exactly the same composition as the epidermis; but have a firmer texture. And of this nature are also hoofs, talons, claws, and such other parts, as in the brute creation answer the purpose of nails.

The *Hair* is of a peculiar composition. HATCHETT, and ACHARD have made several experi-

ments upon it, and VAUQUELIN has given us a complete examination of the subject. The result of his analysis was, that the mass of the hair is insoluble both in cold and boiling water, but that it may be dissolved in PAPIN'S digester: the fluid thus produced does not form itself into a jelly, but becomes viscid as it dries. In this solution, and also when the hair is dissolved in a very diluted caustic lye, or in nitric acid, an oil is produced, which has the colour of the hair. From this very oil, VAUQUELIN deduces the different colours of the hair, and he found in the ashes of black hair, besides the usual salts of lime, oxyd of iron and of manganese and silica. Red hair, which contains more sulphur than the black, left less iron and manganese in the ashes; and white hair left still less, but its ashes were found to contain a distinguishable quantity of magnesia.

Urine has undergone more chemical examination than any other animal matter. VAN HELMONT gave us the first experiment upon it in his treatise on the stone. BRANDT and KUNKEL, twenty-five years afterwards, discovered phosphorus, which they prepared from the constituents of urine. BOYLE, in consequence of their discovery, tried to analyse this fluid, and actually

produced phosphorus from it, which he afterwards caused to be prepared by HANKWITZ, an Apothecary in London, for the natural Philosophers of Europe. About the same time, another analysis was instituted by LORENZO BELLINI, an Italian, who found urine to be composed of water, earth, and salts. It was afterwards examined by BOERHAAVE, whose analysis, considering the time in which it was made, was an excellent performance. Several others, such as MARCGRAFF, POTT, HAUPT, SCHLOSSER, SCHOCKWITZ, BERGMAN, KLAPROTH, &c. occupied themselves with examining the phosphates, which are contained in urine, and endeavoured to improve the methods of preparing phosphorus from it. The younger ROUELLE'S analysis of urine is even at this day of very considerable merit. He discovered its peculiar and characteristic constituent, which he called saponaceous extract; he explained what salts are contained in it, and compared the urine of men with that of herbivorous animals, showing that the latter did not contain phosphates, but carbonate of lime, and benzoic acid. Some years afterwards SCHEELE discovered, that the urine of man contained phosphate of lime, dissolved in an excess of acid, *uric acid,* which till his time

was unknown, and benzoic acid, which, however, for the most part occurred only in the urine of children. CRUIKSHANK, who was employed by ROLLO to examine diabetic urine, instituted afterwards an exact analysis of human urine, as well in its healthy state, as in various diseases, and hence very important results were obtained. He described the urea—showed its property of being precipitated by nitric acid, and stated certain methods to determine with precision the relative quantities of the different constituent parts. His experiments on the urine of sick persons have given Physicians very good diagnostic signs, of which none, who practice medicine, ought to be ignorant. Thus he found, for instance, that urine, during the state we call fever, acquires the property of being precipitated by corrosive sublimate. In a higher fever it is precipitated by alum, and in still higher degrees, by nitric acid. In common dropsy he found albumen in the urine in considerable quantity, as also, though in less quantity, in indigestion; but, on the other hand, its quantity was not increased in encysted dropsies, &c. CRUIKSHANK'S work was published in 1797, as a part of ROLLO'S treatise on Diabetes mellitus. Three years afterwards, FOURCROY and VAUQUELIN published a still

more extensive and very accurate analysis of the urine, and FOURCROY asserts on this occasion, (in his Systeme des Connoissances Chymiques) that he and VAUQUELIN had known the circumstances, which constituted the most remarkable part of CRUIKSHANK'S analysis, several years before that Chemist. FOURCROY and VAUQUELIN examined the phenomena of the putrefaction of urine, and the decompositions and new combinations which take place in the process, and their work was the most complete analysis in Animal Chemistry, at that time known. PROUST has since made further experiments on urine, and has found in it carbonic acid, carbonate of lime, and a peculiar resin, like that of the bile, which, however, seem all to have been formed in the operation. THENARD has since endeavoured to show, that the free acid in the urine is not phosphoric acid, but acetic. Last of all, I have taken up the investigation of this subject, and have obtained results which have escaped the attention of my predecessors. I found that the free acid of the urine was not acetic, nor phosphoric acid; but that it belonged to two acids peculiar to the body, viz. the lactic and the uric acid; and I have proved the presence of the former in urine, by several experiments, which I

think tolerably decisive. In the bone-earth, which is held in solution by the free acid of urine, I found, as in the bones, fluate of lime, and by a comparison with the composition of the blood, it appeared, that the kidneys, in the formation of urine, oxidate a portion of the more remote constituent parts of the blood, and produce several acids, alkalies and earths, which were either not found in the blood before, or existed in it only in a smaller quantity, Thus, for instance, I found in the urine a considerable quantity of sulphuric and phosphoric acid, the former of which is not discernible in the blood, and the latter only in a very minute quantity. The portion of earthy and alkaline salts, which urine contains, is also very considerable, whereas in the blood it is but small. The different sediments, which the urine precipitates whilst cooling, I found to be either solely the mucus of the bladder, which is always present in urine, partly suspended, and partly dissolved, or a combination of this mucus with the uric acid; but it does not contain earthy phosphates. I have endeavoured to show the necessity of making a distinction between the mechanical sediment, which is abundant in the catarrhus vesicæ, and comes from the bladder, and that, which takes place when the bone-earth is

precipitated in the secretion of the urine, for want of a sufficient quantity of acid to dissolve it. The urea, which my predecessors had described, I found to be a composition of the urea, properly so called, and several deliquescent substances, which they had not succeeded in separating from it. The urea, such as I obtained it in my experiments, is void of colour, and forms very distinct prismatic crystals like nitre. It is, however, very obstinately combined with lactic acid, lactate of ammonia, and the above mentioned animal matter, which always accompanies this acid and its salts, and which probably is also always formed at the same time with them. This animal matter has a brownish yellow colour, and in combination with the lactic acid and its salts gives to the urine its colour. It is easily dissolved both by alcohol and water, and from this substance, and not from albumen, as was formerly supposed, arises the precipitate, thrown down from urine by tanning. The matter, which in urine precipitates corrosive sublimate, is not only albumen (the presence of which in case of disease is very probable) but also a peculiar animal matter, which is not dissolved by alcohol, but which, like that soluble in alcohol, always accompanies the lactic acid and its salts. It is not

precipitated from the urine of persons in health as long as it contains free acid, and the want of acid has, perhaps, some share in the precipitation, which the sublimate produces in the urine of persons in a feverish state. Besides these animal matters, the properties of which my predecessors had failed to discover, I also found a mineral substance, which had been overlooked by them, viz. silica. This earth is met with in urine, and probably in all the fluids of the body, though in very small quantities, and has been introduced with the water, which we use partly in the preparation of our food, and partly as a daily beverage, and since no processes occur in the body, capable of separating it, it must accompany the fluids until it be evacuated by the excretions.

The diseased alteration in the urine, which generates saccharine matter instead of urea, and which was so fully examined by CRUIKSHANK, has since been still further investigated by NICOLAS, SORG, THENARD, BOSTOCK, &c. Their analyses vary as to the results, and show, that this diseased state is not always of the same nature; but they all agree, that the new-formed sugar is very easily destroyed during the experiments,

and that it cannot be discovered in the blood. I have had an opportunity of examining diabetic urine, in which no sugar could be discovered, and in which, nevertheless, no urea was found. When alcohol was poured on this urine, when it had been gently evaporated, it dissolved a brown substance, which, after the alcohol was evaporated by heat, left a quantity of stiff extract, in which lactic acid was discovered, and together with it some signs of lactate and muriate of ammonia; the extract, however, consisted almost entirely of the animal matter, which accompanies the lactates, it was precipitated by tanning matter, and left after combustion a small quantity of muriate of soda, which had some slight traces of a free alkali.

The urine of various kinds of animals has been examined by Rouelle, Fourcroy, Vauquelin, Brande, Chevreul, &c. and to these experiments may also be added the analyses made by Fourcroy, Vauquelin and Klaproth, of a collection of the excrements of a bird from the South sea, called Guano, in which they found a great quantity of uric acid. Brande thinks, that he has discovered the same in the urine of the camel, and Vauquelin found it in

the stone from the bladder of a tortoise; from which it appears, that man is not the only animal in whose body this acid is generated.

The *Kidneys*, in which the urine is formed, have not been examined, and the chemical properties of their parenchyma are not known. The urinary bladder and its channels are similar in their composition to the intestines; but the mucus of the bladder, such as it is deposited with the urine, is very unlike the mucus in other places. It is almost completely pellucid, forming small grains, which, when gathered on a filter, leave a slippery and colourless mucus, which, however, often becomes red in drying, and shows signs of containing uric acid. After being moistened with water it does not again become viscid.

The concretions, which are formed in the urine, have, from the remotest times, been the object of conjectures and experiments. From GALEN to PARACELSUS, the ideas relative to these concretions were absurd. VAN HELMONT compared them to tartar; and after him, they were variously described by a great number, among whom, HALES, BOYLE, BOERHAAVE, and SLARE, deserve to be mentioned. At last, some calculi

from the bladder became an object of investigation to our never-to-be forgotten SCHEELE. He soon discovered the uric acid, described its properties, and as he found it in all healthy urine, he concluded, that this acid was always the principal constituent of these calculi. HENRY afterwards extended our knowledge of this acid still further, and SCHEELE has been followed in his investigation by several others, such as AUSTIN, WALKER, BRUGNATELLI, and PEARSON.—At last, Dr. WOLLASTON published, in the Philosophical Transactions for 1797, his analysis of gouty and urinary concretions, which he proved to be of four principal kinds, viz. such as consist of uric acid, of double phosphate of magnesia and ammonia, of oxalate of lime, and of phosphate of lime; and he gave at the same time a short description of their external form, and characters. In the year 1800, or three years after WOLLASTON, FOURCROY and VAUQUELIN published a more comprehensive work on these concretions, in which WOLLASTON'S discoveries were confirmed, but without his being mentioned either in that work, or in FOURCROY'S Syst. des Connoissances Chymiques, among those who had laboured in that field. But, notwithstanding this omission, to WOLLASTON the ho-

nour is due of having been the first to discover the different constituent parts of these concretions; however, as the French Chemists had near 600 different stones to analyse, and before the work was published, had an opportunity of comparing it with WOLLASTON'S, it does contain very important additions, and presents us with a number of different modifications of these substances in the composition of stones. Besides the constituents, which WOLLASTON had discovered, they found two others, viz. urate of ammonia, and silica, the latter of which was only found in two instances; and the former, BRANDE has lately endeavoured to prove to be nothing else than uric acid united to urea; but I cannot consider his reasons for this opinion as completely satisfactory. FOURCROY and VAUQUELIN tried, whether it might not be possible to remove the stone without an operation, by injecting into the bladder either very dilute acids or alkalies, accordingly as the stone consisted of uric acid or earthy salts; but I do not know that this experiment has been attended with any success. They also tried to discover the circumstances, which gave rise to the generation of calculi from urine; this, however, still remains a secret, and we know no more about it, than what

a long medical practice has taught us, respecting the more or less noxious effect of the diet of the Patient. It was accidentally discovered, that the alkalies, when taken inwardly, had a good effect in relieving the pain, in cases of the stone, and that vegetable acids aggravated them whenever the calculi consisted of uric acid; but, notwithstanding this experience, it is often impossible to diminish the acid of the urine by the use of alkali in those that suffer from an excess of uric acid; and I have myself tried in vain the effect of acids in neutralising or acidulating an alkaline urine. A middle-aged man was laid up with the gout, his urine was foul and alkaline, holding the earthy phosphates suspended in an undissolved state. I gave him the sulphuric acid, without any change; and afterwards the phosphoric, without any effect, until its dose was so much increased that it became laxative: the urine then became acid, and deposited uric acid as long as the laxative effect continued, but no longer, although the dose of the acid remained unaltered; lastly, I tried the acetic acid with as little success.

Brande has lately attempted to prove the inefficacy of alkalies as a remedy, in cases of calculi formed of uric acid. Dr. Henry, who had

found that alkaline urates do not precipitate muriat of magnesia, which earth consequently produces with this acid a salt easily soluble, proposed that an experiment should be made with magnesia; which experiment, according to BRANDE'S statement, completely succeeded, so that after using, for two weeks, from fifteen to twenty grains of magnesia, morning and evening, all the superfluous uric acid was removed, and the patient became completely cured. This discovery is as yet too recent to be considered as sufficiently confirmed by experience*.

* Dr. Wollaston has described, in the Philosophical transactions for 1810, a rare species of calculus, containing a substance to which he has given the name of the cystic oxyd. These calculi are in the form of a mass "confusedly crystallized," yellowish, and semitransparent: they are insoluble in alcohol, and in the acetic, tartaric, and citric acids, nor are they affected by the saturated carbonate of ammonia: but they are dissolved by the stronger acids and alkalis: with the acids they afford slender spicular crystals, radiating from a centre; with the alkalis small granular crystals; and their peculiar substance seems to be exhibited alone in the form of hexagonal plates, when slowly precipated from a solution in pot-ash, by distilled vinegar. Dr. Wollaston finds a greater quantity of uric acid in the dung of birds, in proportion as they feed more on animal food; and hence suggests, that the tendency to deposit the uric calculus, or to form gouty concretions, might very possibly be obviated by a vegetable diet. T. Y.

Of the processes, connected with the propagation of animals, and the matters by which they are influenced, we know, as yet, but very little. The seminal liquor has been examined by VAUQUELIN. Its composition begins to change immediately after emission; it deposits by degrees earthy phosphate, which probably results from its decomposition. Its characteristic constituent, which in the first moment appears to be mucus, becomes, when out of the body, thin and liquid in all kinds of gas, and even in a vacuum; the humour, which in the beginning was alkaline, becomes acid by degrees. Of the seminal liquor of other animals, we know only the soft roe of fishes, by means of the experiments of FOURCROY and VAUQUELIN; and in that a peculiarly remarkable substance occurs, which is insoluble in water and spirits; on being distilled in close vessels it produces phosphorus, which is partly sublimed, and partly dissolved in the empyreumatic oil. The substance itself contains neither free phosphoric acid nor any phosphate.

The substances in the female, on which the seminal liquor operates, are entirely unknown, and the chemical nature of these processes is as completely concealed from us, as the intimate

economy of all other processes in Animal Chemistry. The phenomena, under which the fœtus is formed and grows, belong more properly to Anatomical than to Chemical Physiology. Even the manner in which the fœtus receives its sustenance and increase is perfectly enigmatical, since we know with certainty, that the blood-vessels of the fœtus have no immediate connexion with those of the mother.

The fluid by which the fœtus is surrounded in the womb, and, which is discharged at the birth, has been examined by VAUQUELIN and BUNIVA; and according to their analysis, the liquor amnii in women seems to have a great analogy with the fluid of serous membranes, and with the humours of the eye: it contains no more than from $1\frac{1}{4}$ to $1\frac{1}{2}$ p. c. of solid matter. In several of the mammalia urine is formed in the kidneys of the fœtus, and is carried off, through what has been termed the urachus, into a peculiar reservoir, which is formed by the allantoid membrane. When the fœtus is born, the urine mixes itself with the liquor amnii, from which circumstance it was long supposed, that the difference, for instance, between the liquor amnii of a woman and that of a cow, was very consi-

derable. DE ZONDI has, however, lately shown, that these differences relate, properly speaking, only to the urine of the fœtus, which is contained in the allantoid membrane. VAUQUELIN and BUNIVA, who examined these mixed humours, found in them a peculiar cristallizable acid, scarcely soluble, which they called amnic acid. It greatly resembles the benzoic acid, but differs from it in this respect, that it is destroyed both by distillation and by the nitric acid. They also found in it a peculiar brown substance, resembling an extract, which is soluble in alcohol, cannot be precipitated by tanning, and which, consequently, is unlike other animal matters of the same kind. *Meconium* has only been examined by BAYEN, and seems to be a biliary matter, converted into resin, the properties of which the more resemble the bilious matter in the intestines of full-grown persons, the further it has advance from the gall-bladder of the fœtus towards the rectum.

The *Milk* was first examined by BOYLE; after him, BOERHAAVE gave an explicit analysis of it. HOFFMAN, MACQUER, and SPIELMAN, followed; and lastly, ROUELLE and SCHEELE investigated the salts and other less-known con-

stituent parts of milk. SCHEELE now discovered the saccholactic acid and the lactic acid, and demonstrated several chemical properties of the milk. A considerable time afterwards, milk and lactic acid were examined by BOUILLON-LAGRANGE and by THENARD, who declared the acid to be the acetic, united with a peculiar animal matter, from which it could not be separated by distillation. FOURCROY and VAUQUELIN gave us after this an ample analysis of milk, in which they still further rejected SCHEELE'S lactic acid, because its salts, when distilled with sulphuric acid, produced an empyreumatic acetic acid; but as this is the case with several other fixed vegetable acids, which still are not considered as acetic acid, the arguments of the French Chemists seemed not satisfactory. I have, myself, also endeavoured to ascertain the composition of this substance, and have obtained results, which had escaped the attention of my predecessors. I have proved that, in the formation of butter, air is absorbed, and not disengaged, as some Chemists assert, this disengagement only taking place, when the milk is in a fermenting state, and has become impregnated with carbonic acid gas. I have further proved, that cheese occurs dissolved in the milk in a

clear solution, and not merely suspended in an emulsion, and I have endeavoured to ascertain in what it differs from albumen, which it otherwise resembles, and with which SCHEELE compared it. I have also proved, that milk does not contain gelatine; and that the extractive matter, which gives a brown colour to the sugar of milk, resembles what occurs in the humours of the muscular parts and in urine, and consists of lactic acid, and alkaline muriates and lactates, together with the extractive animal matter, which is soluble in alcohol. I have examined the greater part of the lactates, and have proved, as I hope, beyond contradiction, that the acid cannot be either the acetic, or any other vegetable acid, but is a peculiar and very remarkable acid, which occurs in the economy of the animal body, and for which I have retained the name of lactic acid, which it received from its discoverer, although it is found in greater or less quantity, as well in the juices of the muscular flesh, as in urine; and I have thereby restored to our illustrious countryman SCHEELE, the singular honour of never having advanced an erroneous statement relating to the Science of Chemistry.

Finally, with regard to the manner in which I have endeavoured to treat the subject of Animal Chemistry, it has been altogether different from that of my predecessors, who, considering it as a part of general chemical knowledge, have all divided the productions of the animal body into certain classes, and described them only as objects of analytical chemistry, to which they have added an appendix, with some general reflections on the economy of animal life. But this mode of treating Animal Chemistry is altogether without an object, and gives to the results of chemical investigations, little more than a technical value, which, however, is entirely foreign to Animal Chemistry, properly so called. For my part, I have endeavoured to unite chemical and anatomical researches in the pursuit of one common object, in order thus to give to the investigation of the Animal Chemist, a determined and scientific tendency, and to his efforts, a physiological view. As my predecessors have not always begun from the same point, or taken their aim in the same direction, it has happened, that much has been overlooked by them, which might have been found without difficulty, and thereby I have been enabled, in the experiments which I have

had an opportunity of instituting, to discover or prove many circumstances, till then either unknown or imperfectly stated, but of importance to the physiologist : and I foresee, with pleasure, that, when more able men than myself shall hereafter occupy themselves with researches in Animal Chemistry, in the same manner as I have done, this interesting Science will acquire a degree of perfection, which, at present, we not only do not expect, but scarcely even venture to hope.

FINIS.

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